

Yet Another Lame Duck Survives

THE British government's conversion to the principle that an industry in trouble should have support from public funds is now almost complete, but the Conservative government seems to be no clearer than its Labour predecessor about the reasons for its generosity. Early this week, Mr Christopher Chataway, the Minister for Industry, announced that the government is to invest £4.9 million in a new company formed from the Norton-Villiers Division of Manganese Bronze Holdings Limited and Birmingham Small Arms Limited with the objective of manufacturing motor cycles. The need for this investment has come about because the Birmingham Small Arms company, which has long since exchanged its manufacture of rifles and other military weapons for the outwardly pacific but hardly less offensive trade of manufacturing motor cycles, has had two disastrous years in a row and has reached the point of collapse. The Norton-Villiers organization, by contrast, has been relatively successful in the past few years, recovering slowly but steadily from the collapse of its American market when, apparently to everybody's surprise, Japanese motor cycle manufacturers began selling motor cycles with large engines which were technically superior to those of their British competitors. What Mr Chataway said this week is that the government's investment in the new company is intended to secure an export market that could be worth £30 million a year in the decade ahead.

Several questions need urgently to be asked of the government. First, if the export of motor cycles is considered to be strategically necessary—and there is room for argument about that—might it not have been better that the public money now committed should have been invested in the successful company and not in its amalgamation with the unsuccessful company? No doubt the government is anxious to avoid the loss of jobs that would result from the complete collapse of BSA, given the several frights about the level of unemployment received last winter, but on this occasion it need not have worried. An investment in the successful company would surely have meant that the relevant parts of BSA, and the workers concerned, would have been snapped up. As things are, the shareholders, the management and the labour force of BSA will be protected from the knowledge that they have failed. And the government will have lost yet another chance of making good its persistent failure to recognize, in the recent history of public investment in British industry, that it is better to invest in successful enterprises than Mr John Davies's now proverbial lame ducks.

The second question, on which both the Labour and Conservative governments have been repeatedly caught out, is whether the new enterprise will be viable even with the government investment and the £10 million which it is hoped that the banks will now lend. For there is no secret behind the success of Japanese motor cycle manufacturers in the American market in the past few years. By the use of multi-cylinder engines, overhead camshafts

and such devices as electric starting motors, they have been able to sell products which are in many ways superior to those of British manufacturers. In the years ahead, the technical competition is likely to be still fiercer. It cannot be long before Japanese manufacturers are marketing machines which use rotary engines. It is vitally important to know whether the new company now put together will be able to support research and development on the scale necessary to make it competitive in the 1980s. If it cannot, the outcome could be yet another failure in the not too distant future.

Yet another question which the government must answer is whether investment in motor cycle manufacturing is strategically worthwhile. It is not, after all, as if Britain is starved of opportunities for industrial investment. Indeed, there are several other ways in which an export business in less marginal products than motor cycles might be built up. To be sure, the government is worried about employment, while its hands are to some extent tied by the inflexibility of the British labour force. People brought up to manufacture motor cycles seem slow to recognize that they can just as well help to manufacture other kinds of industrial products, not merely motor cars but vacuum cleaners, refrigerators and the like. That said, however, there is also an urgent need for a re-examination of the government's plans for industrial retraining. Is it not high time that more efficient machinery for converting people to technologically advanced work was implemented? It could easily turn out, on a narrow accounting basis, that Britain would get better value for the money the government is now proposing to invest in making motor cycles by using it, instead, for sensible training programmes. In the long run, however, the only sustainable objective is that there should be a programme for the development of British industry which will provide not merely a skilled and adaptable labour force but a coherent strategy for deciding what should be manufactured. There is nothing wrong with the mechanism which exists already by means of which commercial institutions decide whether or not to risk money in new enterprises. The government's intervention, in the BSA case as well as in the rescue of other lame ducks in recent months, has blunted this machinery.

Princeton in Trouble

THE Institute of Advanced Study at Princeton is one of the most distinguished of all academic institutions in the western world. The tiny faculty of twenty-six has been able to exercise a profound influence on the development of scholarship, partly because of the quality of its members (including Einstein, von Neumann and Oppenheimer) and partly because of the historical accident that the institute functioned, in the 1930s, as a splendid

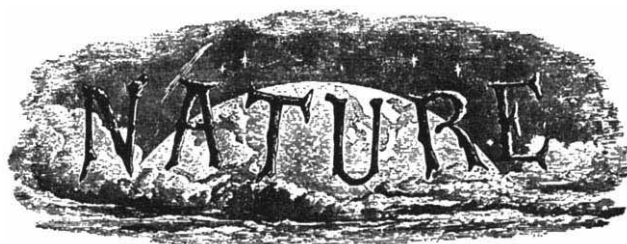


channel for preserving and transporting to the New World the academic traditions of Western Europe, Germany in particular. Since then, the institute has been a powerful stimulus for research in mathematics and the physical sciences and, though it has no formal connexions with undergraduate teaching institutions, the steady stream of visiting fellows through Princeton has helped enormously to enliven universities in the United States. In other words, the Institute of Advanced Study has been, for forty years or more, a powerful demonstration that ivory towers can still function efficiently. It is therefore a great misfortune that it should now be embroiled in an academic quarrel which, if the faculty does not watch its step in the next few weeks, could easily undermine much of what has been accomplished in the past forty years.

The facts are these. Dr Carl Kaysen, who succeeded Oppenheimer as director in 1967, has let it be known from the start that he would add a social science faculty to the three which have been there from the beginning—mathematics, physics and history. In 1970, Professor Clifford Geertz of the University of Chicago was appointed as the first tenured member of a social sciences faculty, apparently without dissent. More recently, there has been argument about the appointment of Dr Robert N. Bellah, a sociologist from Berkeley, to the same faculty. Dr Bellah is a specialist in the influence of religious symbolism on social structures. His proposed appointment to the faculty, advocated by Kaysen and Geertz, was considered by the combined faculty of the institute on January 15 and voted against by a margin of thirteen to eight. It seems to be agreed that while the recommendations of individual faculties of the institute on proposed internal appointments should be for all practical purposes mandatory, the faculty as a whole has only an advisory function in recommendations to appoint to newly established programmes such as that in the social sciences, which is why Dr Kaysen was within his rights in recommending to the trustees of the institute that Dr Bellah should be appointed. The outcome has, however, been unpleasant. For one thing, Dr Bellah's work is now being openly criticized by the faculty, many members of which are advocating a measure of faculty power that would give them ultimate responsibility for all tenured appointments. There is also an undercurrent of complaint at Dr Kaysen's administration of the institute and a group of fourteen members of the faculty have asked that there should be an outside scrutiny of "the director's stewardship", as a result of which a panel of the trustees is planning to hear views on this controversial subject this Saturday, March 24. Nobody would be surprised, but most people would be disconsolate, if the outcome were, now or at some time in the future, the director's resignation.

This is why it is important that there should be a better sense of moderation in this kind of academic community. Whatever the merits or defects of Dr Bellah's scholarship, it is absurd for anybody at Princeton to suggest that a single appointment can entirely change the character of the faculty. Of that there can be no dispute. Good institutions are precisely those which should be able to accommodate unevenness. The central issue is whether a faculty of twenty-six (enlarged to twenty-seven by Dr Bellah's presence) should have the close degree of control over all academic appointments which the dissidents at Princeton seem to want. It is hard to avoid the impression

that there is an element of dissent from Dr Kaysen's plan for a social science programme in what the dissident members of the faculty have been saying. For several years, the new director has not troubled to conceal his impatience with the narrowness of the institute's chief lines of inquiry, and it is entirely proper that if a new faculty is to be created in the social sciences, he should have an important say in its composition. From the outside at least, what the faculty is asking for is a degree of control over academic innovation which is in many ways intolerant. Unless the dissident members are somehow able to make their arguments less pernicious, they will find that they have done more damage to the institution they say they wish to defend than any number of unsatisfactory appointments might accomplish.



SURVIVAL OF THE FITTEST

THE doctrine of the "survival of the fittest" must be strangely understood in some quarters. The American papers report Prof. Agassiz as having expressed himself in this wise at a recent meeting of the Massachusetts State Board of Agriculture, of which he is a member:—"I do not know how animals originated; a brilliant imagination that of Darwin; a very necessary faculty in the scientist. The sense I know too well to misquote him. Hasty generalising of observation is Darwin all over. Natural selection is out of generation. Natural necessity, what is it? Do we find that only the strong beget families? *Observe plants at the foot of the White mountains, where are large trees, and so up to the summit, where they are mere shrubs.* The weak may and do survive as well as the strong. Ignorance lies at the base of the discussion."

Probably no one naturalist, however eminent, can be expected to know everything, or even all simple things. Can it be possible that Prof. Agassiz supposes (as his argument seems to require) that the dwarf trees in question grow and survive near the top of the mountain, *notwithstanding* they are not the fittest, rather than *because* they are the fittest, for the conditions? And does he conceive the doctrine of natural selection to be founded upon some idea of an abstract fitness, irrespective of the conditions, and not upon the survival of the fittest under and in consequence of the conditions? Surely the argument brought against the doctrine is a good illustration in its favour, only an extremely simple and elementary one.

We never could quite comprehend why Prof. Agassiz should give himself so heartily and persistently to the work of demolishing the doctrine of the derivation of species, in all its forms, considering how large and honourable a part he has himself taken in laying the foundation upon which the modern doctrine has been built. Of these foundations none is stronger than the capital one, generally supposed to be established by him, that the succession of species in time corresponds mainly with that in systematic rank, and is also somehow paralleled in the development of each individual of the higher ranks. So that, in view of his continued but unsuccessful efforts to drive the incoming doctrine out of the land, we could imagine him addressing his own important discoveries in the words used by Balak to Balaam:—"What hast thou done unto me? I took thee to curse mine enemies, and behold, thou hast blessed them altogether."

OLD WORLD

End of the Road in Sight for Nina

NINA, the 5GeV electron accelerator at the Daresbury Nuclear Physics Laboratory will have to be closed, probably in five years time. It has been realized for a long time that in order to keep the Science Research Council's spending on nuclear physics constant, either Nina or Nimrod, the 7GeV proton synchrotron at the Rutherford Laboratory, would have to stop operating when the 300 GeV accelerator at CERN became fully commissioned.

The announcement that Nina is to be scrapped was made by Sir Brian Flowers last week at the University of Newcastle. But this does not ensure the future of Nimrod for an indeterminate period for, apart from other considerations, the machine will be seriously outdated by the end of the 1970s.

The future of both high and low energy nuclear physics research in Britain for the second half of the 1970s is now taking shape. On the low energy front the Science Research Council's request for money to build a Nuclear Structure Facility is still with the Department of Education and Science and it looks very much as if the facility will now be built at Daresbury if the government approves the expenditure of £4 million over 5 years. High energy physics will be centred in Geneva with the 300 GeV accelerator while Nimrod will be used as a valuable back-up machine until it is probably phased out at the end of the decade.

But nuclear physics did not occupy all of Sir Brian's speech and his theme was what the chairman of the Science Research Council can and cannot do. In a frank revelation of the chairman's decision making powers, Sir Brian, in his last few months at the council before he becomes rector of Imperial College, covered most of the major decisions made during his tenure of office.

Sir Brian gave a detailed account of the series of decisions that culminated, eventually, in the British government agreeing to participate in building the 300GeV accelerator at CERN. He also gave a similar account of the not so successful attempt by the Science Research Council to persuade the government to provide money to build a high flux beam reactor in Britain. Sir Brian, modestly, pointed out how astrophysics research at the Culham Laboratory was taken over by the SRC from the United Kingdom Atomic Energy Authority and how, diplomatically, the rival claims of Sir Martin Ryle and Sir Bernard Lovell



Professor Sir Brian Flowers, Chairman, Science Research Council.

in 1968 for money to build respectively, a radio telescope at Cambridge, for £2 million and a giant new dish at Manchester for £5 million, were resolved in Sir Martin's favour. But there was a

consolation for Sir Bernard in money granted to improve the telescope at Jodrell Bank. Now, of course, it is Sir Bernard's turn. The Cambridge radio telescope is operational and plans are well advanced to build a new telescope in mid-Wales for the University of Manchester Nuffield Radio Astronomy Laboratory.

Sir Brian also pointed out how the attempts of the Science Research Council to shift the balance in post graduate training from science to engineering is being frustrated by the universities. During the past few years the SRC—as a matter of national policy, says Sir Brian—has been transferring its studentships from science to engineering, but the universities with equal vigour have been transferring the studentships not under the control of the SRC from engineering to science. This, says Sir Brian, is one of the “principles of frustration” arising from the dual support system and demonstrates how the power of a research council is limited by its abilities to persuade. So postgraduate engineering training has increased at a much slower rate than the SRC wanted it to and postgraduate

SOVIET SCIENCE

Planetokhods Planned

from our Soviet Correspondent

As Lunokhod-2 continues its successful programme of investigations in *Mare Serenitatis*, an interview in *Pravda* with Academician A. A. Blagonravov suggests that plans are already being made in the Soviet space programme for the automatic exploration of the planets by similar travelling laboratories. Although some of his remarks relating to the possible “planetokhod” exploration of Venus and the outer planets seem highly speculative, the possibility of a “Marsokhod” seems to have reached the stage of constructive planning.

The principal difference between the lunokhods and the projected Mars vehicle would be in the manner of control. The lunokhods are controlled from Earth—a Mars vehicle would have to be self-controlling at least as far as such vital commands relating to its motion as “start” and “stop” are concerned. Certain instrument units would also have to be autonomic, in order to investigate immediately any specific feature encountered during motion. Academician Blagonravov does not,

however, speak of a fully automatic vehicle, and it would appear that there would be some Earth control—perhaps in the activation of data transmission sequences.

The Marsokhod plans are concentrating on increasing the distances the vehicle can travel quickly rather than on extending its life span. This seems to have been dictated by communications considerations—a rapid, productive survey close to conjunction being preferable to the slow, long-term surveys carried out by the lunokhods.

The Martian atmosphere is envisaged as presenting “difficulties” for such a vehicle, and special protection will have to be included against dust penetration and friction of the moving parts. This, however, is essentially a matter of modifying the vehicle itself and its external sensors, since the instrument units proper operate in a sealed compartment with pressure and temperature close to Earth-normal. In Academician Blagonravov's view, the lunokhods may be considered as prototypes for future Mars craft, and the successful landing of a Marsokhod on Mars itself in the “fairly close future”, he says, “cannot be doubted”.

science training is still being maintained at a level that is difficult to justify at present.

In spite of this condemnation of the principle of dual support Sir Brian says that it is important that the universities be not wholly reliant on the research councils for support for their research. The SRC is perfectly right "to decide that it can only afford to support (say) five major university centres in a given topic," said Sir Brian "or to support only (say) 20 postgraduate schools of chemistry and to let the rest go hang". Then universities which were really determined "to achieve greatness" could spend their own money to achieve it and become one of the accepted universities at the expense of one of the others already on the list.

NUCLEAR POWER

Plans for the Future

By 1985 it is planned that 33 per cent of the electricity generated within the European Economic Community will arise from nuclear reactors. The European Commission recently approved a plan that sets as an objective a minimum output of 100,000MWe from nuclear power plants by the mid-1980s which will amount to ten per cent of the total energy needs (including oil and gas) of the community at that time.

At present the total community nuclear power capacity is 5,500MWe of which 3,250MWe are generated in England and Wales by the Central Electricity Generating Board. If plants under construction are taken into consideration the total community output in 1975 will be 12,000MWe and about 23,500MWe in 1977. In Britain five Advanced Gas Cooled Reactors are being built with a total capacity of about 6,500MWe which will be operational by 1980. These reactors are of course the first AGRs to be built.

The total electricity output in England and Wales is 56,000MWe at present which is projected to rise to 160,000 MWe by the end of the century. The CEGB estimates that one half of this will be produced by nuclear power.

If the European Commission's objective of 100,000MWe output from nuclear power in 1985 is to be realised then 45,000MWe should be produced by 1980. This, according to the community's Nuclear Information Programme, implies that 7,000MWe a year have to be added between now and 1975 and 11,000MWe to 12,000MWe a year between 1976 and 1980.

On a longer time scale the Nuclear Information Programme estimates that in 1990 the nuclear power output of the community should be 210,000MWe and 620,000MWe in 2,000.

But the community is not completely

ignoring the claims made by the coal industry for coal to be given its place in the energy equation. The European Commission has already issued a draft of its scheme for aiding the iron and steel industry within the community in buying coking coal and cokes. This draft is now before the Council of Ministers awaiting approval.

The British Coal Industry will no doubt welcome the community's interest in coal. When Britain joined the EEC in January the community's coal output increased from 170 million to 300 million tons a year. Late last year (see *Nature*, 240, 516; 1972) a document, *Coal and Energy Policy in Europe*, was produced by the National Coal Board, the National Union of Miners and other bodies which called for coal to be used to "its full and effective potential" within the European Community. This report pointed out that by 1980 there will be a gap between the energy produced within the community by natural gas, north sea oil, lignite, nuclear power and hydroelectricity, and the requirements at that time. This gap, which the report estimates to be almost 1,200 million tonnes of coal equivalent, will be filled by coal and imported oil. The plea of the British report was that "as large a part of this market as possible be supplied with coal".

VIVISECTION

Fund for Research

A FUND to encourage research techniques that do not involve vivisection has been set up. Following a two-day seminar held in London last week on the potential of humane non-animal research techniques, the National Anti-Vivisection Society announced that it was providing £10,000 to start the fund.

Known as the "Air Chief Marshal the Lord Dowding Fund for Humane Research", the fund will provide prizes or grants for scientists carrying out research "likely to lead to the alleviation of human or animal suffering, or likely to lead to the advancement of a new discovery which shall be useful for saving or prolonging life". The only caveats are that live animals must not be involved in the experiment at any stage, and applicants must not hold a licence for animal experimentation.

Mr Richard Body, MP, Vice-Chairman of the All-Party Parliamentary Humane Research Group, said that "we hope the grants will also encourage scientists to seek means of evaluating new drugs and medicines without resort to tests on live animals. Quite apart from the suffering inflicted on many of the 5.5 million animals now used annually in experiments in the United Kingdom the use of animal tests for the evaluation of new drugs is so unreliable in many respects that unless

more efficient methods are found many doctors and scientists fear we could be heading for another tragedy on the thalidomide scale".

CONSERVATION

Whale Imports Banned

THE British government has banned the import of the majority of whale products. The ban, which came into effect at midnight on March 15, comes after considerable pressure by environmental groups to get Britain to suit its actions to its words.

Britain has not been an active whaling nation since 1963 and is one of the nations that has supported moves within the International Whaling Commission to cut the whale catch substantially as it has become plain that many species of whales have been overhunted. But, while Britain has argued that whaling should be controlled, whale product imports have gone ahead unfettered.

But last week's ban is not a total one. Mr Anthony Stodart, Minister of State, Department of Agriculture, Fisheries and Foods, announce that the Department of Trade and Industry's studies on alternatives to the use of whale products have shown that some industries can use substitutes for sperm oil but that others will have difficulty finding replacements. The decision, therefore, is to ban the use of all whale products except sperm whale oil used as engine lubricant, spermaceti wax, ambergris and whale products incorporated into manufactured products abroad and then imported.

Friends of the Earth, who have campaigned to prevent whale imports for more than a year now, are well pleased with the decision, although they are eager that the ban should be extended to cover sperm whale oil.

Nonetheless the government's action will produce a considerable reduction in whale product imports. Nineteen thousand tons of whale meat valued at £2.2 million were imported in 1970; by 1972 these figures had fallen to 8.8 thousand tons valued at £1 million. These imports are now banned. The import of whale oil, other than sperm whale oil, which is put at 850 tons last year is also banned.

Although sperm whale oil can still be imported the amounts entering the country have been diminishing over recent years. In 1972 only 7,600 tons were imported, a little over half the amount imported in 1970.

Studies of alternatives to sperm whale will continue, and Friends of the Earth hope to continue their campaign for the complete ban of whale product imports in June when the International Whaling Commission is again due to meet in London.

FORESTRY

Pesticides and Planes

from a Correspondent

AGRICULTURAL chemicals and agricultural aviation, their relevance to the management of forests and their use in the fight against pests and diseases of forest trees, was the theme of a recent symposium of the Agricultural Aviation Group of the Royal Aeronautical Society and the Society of Chemical Industry, Pesticides Group.

The section of the symposium devoted primarily to chemicals was aptly timed as its chairman, Mr D. Bevan (Forestry Commission), noted in his opening remarks. A recent Green Paper (*Forest Policy*, HMSO, June 1972) envisages a wider remit for the two million acres of state-owned forests in the United Kingdom than merely cellulose production. Conservation, wildlife management, amenity and recreation may be added responsibilities in future. These changes may eventually loosen the economic constraints which apply to the use of fertilizers and pesticides in Bristol forests, but Mr J. F. Morgan (Forestry Commission) concentrated his analysis on the potential for these chemicals on a continued need for them to pay for themselves by increased cellulose yield. Discounting their costs during the period of several decades which may elapse before the trees are felled, Mr Morgan showed that the required returns from chemicals can seldom be obtained in practice from cellulose production.

Two reports gave the latest information on the control of Dutch elm disease. Dr J. N. Gibbs (Forestry Commission) described 1972 trials with basal injections of a solution of benomyl directed against the causative fungus, *Ceratocystis ulmi*. He reported hopeful results and estimated that the treatment cost of about £10 per tree would be justified on some 370,000 of the 2.7 million large elms currently at risk in the southern half of England—justified in this case by their amenity value or the high cost of felling if they were allowed to die, and not by future timber production. Promising preliminary results with insecticides against the beetle vectors of the fungus were also reported by Mr T. M. Scott (Forestry Commission).

Two reviews dealt with the use of aircraft in forestry. After describing the situation in Europe, Dr W. J. Maan and C. H. Cottle (International Agricultural Aviation Centre) concluded that use of aerial fertilizers would become more frequent. Broad spectrum insecticides would be replaced by more specific compounds or biological control agents, but in Europe aerial pest and disease control would still be confined, for the most part, to situations where the

survival of the crop was threatened. Applications of herbicide would increase slightly and forest fires would continue to be spotted and occasionally fought from the air. This forecast seemed restricted when compared with the present situation in Canada, which was comprehensively reviewed by Professor P. H. Southwell (University of Guelph). He highlighted the divergence which has occurred in Canada between agricultural and forestry aviation, chiefly because forest spraying frequently involves uniform areas of terrain which require few turning points. Thus relatively fast and heavy ex-military aircraft are used with large payloads, achieving ten or twenty times the rate of work of agricultural operations. Ground-based fire spotting, the meeting was told, has been almost superseded in Canada by airborne infrared patrol techniques and, once located, fires are eliminated or temporarily contained by aircraft.

BRITISH ASSOCIATION

Student Conference Plan

THE British Association is to hold a student conference in January 1974. The four-day meeting, from January 2 to 5, will be centred on the Royal Society and the subject will be the social responsibility of the scientist within the community.

The BA hopes that the conference—the first specifically for students—will form a springboard for its attempts to increase the number of students interested and involved in the association's activities.

In the past the association has woefully neglected the interests of its student membership, which, before last September's meeting in Leicester, numbered only 147. During the Leicester meeting the number of student members rose to more than 200 and the idea of a student conference emerged.

The programme will open with a meeting at the Royal Society which will be addressed by one of the society's officers. On the Thursday and Friday mornings two speakers will lecture during the morning and discussion groups will mull over the points raised in the afternoons. On the Friday evening it is hoped that the groups will report to the conference and discuss their findings with a panel of speakers.

Subjects to be covered include the impact of science and technology on society and the relative allocation of resources to different areas of science.

The British Association hopes that the conference will become a regular item on its agenda and to encourage as many students as possible to attend the association is to subsidize the conference in order to keep the cost for individual students down.

COMPUTERS

Anger at the End

INTERNATIONAL Computers Limited's managing director, Mr Geoffrey Cross, accused the Select Committee on Science and Technology of asking damaging questions in the closing minutes of the committee's inquiry into the computer industry last week.

Mr Ian Lloyd, a member of the committee, had asked Mr Cross and Mr Tom Hudson, ICL's chairman, if they had any comments on the committee's questions. Mr Cross promptly quoted an occasion when Mr Lloyd had asked Christopher Chataway, Minister for Industrial Development, if the £14.2 million the government has given ICL to support its new product range would be sufficient, the select committee having recommended that £50 million support should be given to the industry. This, Mr Cross said, could be damaging and it implied that the company had not got what it needed. In fact £14.2 million was the amount it had requested from the government.

Again, Mr Cross said that ICL had been asked whether it would rather merge with Univac or Nixdorf. "That's a damaging question," Mr Cross said, "because it assumes we'll do one or the other." Mr Cross emphasized that he had made these points to the committee during private session. Mr Airey Neave, the committee's chairman, was clearly annoyed at Mr Cross's action, warning him "to be very careful about what you say in public about private sessions", and suggesting that it was the answers to the questions that might be damaging rather than the questions themselves.

After the hearing Mr Neave made it clear that the private hearing had been at ICL's request, not at the committee's.

Mr Cross also described Mr Christopher Layton's claim that ICL could not survive without government support after 1976 to 1977 as "a lot of nonsense". He had, however, had a meeting with Mr Layton the previous week, and felt that his disagreements with Mr Layton were over matters of detail rather than over fundamentals.

Mr Hudson told the committee that at present the influence of the EEC commission on the computer industry was fairly small, but he admitted that this could change considerably in the "not too distant future" if the commission acquired funds for its ideas.

ICL told the committee that as yet it had not had any contact with the Department of Trade and Industry's computer requirements board which was set up at the end of 1972. The company also said that the British government was not an advanced user of computers and relied on them less than other European governments.

NEW WORLD

Knives Out for Surgeons

by our Washington Correspondent

AFTER a year of being thrust into the public spotlight and being debated in medical journals, newspapers and committee rooms on Capitol Hill, psychosurgery made its first appearance in court last week. Brought before a three-man panel of judges in Detroit, the court case is likely to have important implications for the future of psychosurgery in the United States and by the time it is finished it will probably have exposed a number of other issues as well. The appearance was at a pre-trial hearing, as a result of which the judges have now decided that the case is worthy of a full scale trial. It is set to open in Detroit on March 27, and will probably last for several weeks.

At the centre of the case are a 36-year-old alleged murderer and rapist who has spent the past 18 years in institutions for the criminally insane, and a team of doctors from the Lafayette Clinic, the highly respected psychiatric research centre attached to Wayne State University. The man, known in the court case as Mr L, was to have been the first patient to take part in a project at the clinic designed to provide experimental treatment for violent patients in state hospitals in Michigan, in the hope that they could be returned to society.

The patients, selected on the basis of several factors including their intelligence, length of stay in hospital and inability to respond to other forms of treatment, would have been given a choice between treatment with a drug known to have some side effects—including possible impotence—and psychosurgery, or they could have opted for neither. Mr L, after having been told the risks associated with either form of treatment, opted for psychosurgery—in this case, the destruction of a tiny portion of the brain with deeply implanted electrodes.

He was transferred to the Lafayette Clinic from Ionia State Hospital, but before the operation could take place Mr Gabe Kaimowitz, a civil rights lawyer, filed suit to try to stop it. The case hit the headlines in the Detroit papers, Mr Kaimowitz got an injunction, and the full hearing opened last week. In the meantime, however, the Michigan state government, which was to have funded the project, withdrew its support and so the case as such, no matter what the court decides, is already dead. The court is thus in effect being asked to rule on a number of ethical

issues surrounding psychosurgery in particular, and experiments involving prisoners in general. Its decision is therefore likely to have widespread implications.

Psychosurgery has become a subject of public debate chiefly because of the crusading of Dr Peter R. Breggin, a novelist and psychiatrist attached to the Washington School of Psychiatry. Breggin last year put together a scathing attack on psychosurgery, documented with examples of cases in which it has damaged the intellect or other functioning of patients on whom it has been practised, and his paper was read into the *Congressional Record*. He has suggested that new techniques, such as the use of electrodes, ultrasonics, and the injection of butane gas to destroy tiny areas of the brain, are likely to usher in a new age of lobotomy, and he believes that all psychosurgery

should be banned.

Although estimates vary widely, there are reckoned to be about 500 psychosurgery operations performed in the US each year on a variety of patients. Unlike the classical lobotomy operation, which reached its height of popularity in the 1950s and was used on up to 50,000 people in the US, psychosurgery now rarely involves actually cutting into the brain, but it does have one important facet in common with classical lobotomy—it is irreversible. Moreover, although the new techniques destroy less brain material, there is still no theoretical understanding of the function of the material which is destroyed.

Opponents of psychosurgery also point out that, unlike normal medical practice, the technique is designed to destroy tissue which is neither diseased nor malformed. Psychosurgery, which

APPOINTMENTS

Two Down but More to Go

by our Washington Correspondent

ONE sign of the Nixon Administration's lack of enthusiasm for science and technology is the fact that key positions in the federal scientific bureaucracy have been unfilled for months. But at last a few appointments have been made. Last week, President Nixon announced that Dr Charles C. Edwards, now Commissioner of the Food and Drug Administration, has been nominated as Assistant Secretary for Health in the Department of Health, Education and Welfare—the top health position in the federal government—and that he has nominated Dr Betsy Ancker-Johnson, an engineer and consultant to Boeing, to be Assistant Secretary of Commerce for Science and Technology. The health job has been vacant since Dr Merlin K. DuVal resigned in December, while the Commerce job has been vacant since Dr James T. Wakelin jun. resigned in August last year. Both nominations must be confirmed by the Senate.

The elevation of Dr Edwards to the position of Assistant Secretary for Health has been expected for several weeks, and last week's

announcement has been greeted with approval from most quarters. As commissioner of the FDA, it is generally considered that Dr Edwards did a good job in often extremely difficult circumstances. The FDA is constantly caught in the crossfire between a strong and increasingly more vocal consumer movement and the food and drug industry. Dr Edwards is, in fact, the only senior health official left in the Administration since President Nixon either accepted or forced the resignations of the Surgeon General, the Director of Health Services and Mental Health, the Assistant Secretary for Health and the Director of the National Institutes of Health, but his promotion now creates a vacancy at FDA. No successor has yet been announced, and Nixon said last week that the choice would be made by the new secretary of HEW, Caspar Weinberger. Since the FDA is likely to be broken up with the passage of legislation to create an independent Consumer Protection Agency, Weinberger may have some difficulty finding a good person for the job.

is designed to alter behaviour, is usually distinguished from the treatment of epilepsy and the removal of brain tumours, which, although they may involve the destruction of brain tissue, are performed to combat organic disease.

The debate about the ethical implications of psychosurgery which has been taking place in the US recently has been emotionally loaded, and a set of hearings held by Senator Edward Kennedy last month, as part of his investigation of human experimentation (see *Nature*, **242**, 152; 1973), provided an outlet for many of the arguments on both sides of the fence. A sober and carefully measured appraisal of the situation was, however, given by Dr Bertram Brown, Director of the National Institute of Mental Health.

Brown told the committee that he believes that knowledge of the brain is at present too limited to provide justification for psychosurgery. "My own view", he said, "is that more knowledge and more refined techniques would be needed before one could determine unequivocal clinical indications for psychosurgical intervention". When asked by Kennedy if he believes that psychosurgery is a valid technique for treating behaviour disorders, Brown replied "My answer is a crisp negative".

He went on to say that although the federal government cannot regulate the practice of surgery, the Department of Health, Education and Welfare has published a set of guidelines for any experiments involving human subjects. These embody the principles that the risks to the patient are outweighed by the benefits to him or by the importance of the knowledge to be gained, that the experiment should have been subjected to peer-review and that the patient should be completely informed of the risks and give his consent freely.

In the case being studied by the Detroit court, the project was subjected to two reviews, and Mr L and his parents gave their consent to the operation after being informed of the risks. But a central issue in the court case revolves around the question of whether a person involuntarily detained in a mental institution is in fact in a position to give his consent to treatment freely. That, of course, is an issue in all research involving prisoners of any kind.

NAS

Reorganization Begins

by our Washington Correspondent

THE National Academy of Sciences has begun to implement the scheme for reorganizing its operating arm, the National Research Council, that was agreed at the annual meeting last year

(see *Nature*, **237**, 6; 1972). It was announced last week that an Assembly on Behavioral and Social Sciences and a Commission on Natural Resources have been established, and that two more assemblies and four more councils will be established during the next few months. Dr Robert McCormick Adams, dean of the Division of Social Sciences at the University of Chicago, has been appointed as chairman of the Assembly of Behavioral and Social Sciences, and Dr Gordon J. F. MacDonald, of Dartmouth College, who was a member of the Council on Environmental Quality until late last year, will be the first chairman of the Commission on Natural Resources.

Set up by President Wilson in 1916, the National Research Council is the organization responsible for carrying out the studies contracted by the academy, and it is administered by the Council of the National Academy of Sciences. Its reports, which are reviewed by a committee of academy members, thus bear the prestige of the academy, although they are prepared by committees consisting of scientists drawn from the whole scientific community and they do not necessarily include academy members.

Organized at present along divisional lines which roughly correspond to the chief scientific disciplines, the NRC is increasingly being called upon to perform studies which include inputs from several disciplines, and the reorganization is designed to make it more responsive to such demands.

The plan is to set up two more assemblies—concerned with the life sciences and with the physical sciences and mathematics—which, together with the new Division on Behavioral and Social Sciences, will be concerned chiefly with the welfare of their component disciplines. They will, for example, be concerned with the preparation of such reports as the recent survey of physics. In addition, they will provide manpower to serve on committees set up to study problems undertaken by the commissions. Although ultimate authority for the work of the assemblies will rest in the council of the academy, each will be run by an executive committee composed chiefly of academy members.

The commissions, in addition to that on natural resources, will deal with human resources, peace and national security, international scientific affairs, and "technologies in large, complex social systems" such as transportation and urban development. At present, some 8,500 scientists serve on committees of the NRC, and a statement on the new reorganization suggests that steps are being taken to strengthen selection of committee members so that "potential sources of bias—

whether real or apparent—are not permitted to undermine the credibility of the advisory process".

The reorganization plan has been a long time in coming, for it was first proposed by NAS President Philip Handler soon after he took office in 1969. It has, however, been delayed by a dispute between the National Academy of Sciences and the National Academy of Engineering over governance of the NRC. Although the NRC is the operating arm of both academies, only the council of the NAS has formal control over the affairs of the NRC under the terms of its charter. The engineering academy is unhappy about such an arrangement, and from time to time there have been murmurings that it would sever its ties completely with the NAS. At present they are still together, but the arrangements announced last week do not provide for the NAE to have any more voice in the running of the NRC than it has had in the past.

HEALTH RESEARCH

More Polish Accord

by our Washington Correspondent

ANOTHER scientific agreement between the governments of the United States and Poland was signed last week. This time, the agreement concerned health matters—in particular, the sharing of information and the exchange of scientists working chiefly in nine areas of health research. Signed in Washington by Caspar Weinberger, Secretary of Health, Education and Welfare, and the Polish Minister for Health and Social Welfare, Dr Marian Sliwinski, the agreement supersedes a memorandum of understanding dating from 1962. That memorandum was limited to activities funded with US owned excess foreign currencies derived chiefly from the sale of agricultural products.

The new agreement relates specifically to cooperation in research into cancer, cardiovascular diseases, occupational health, maternal and child health, rehabilitation, neurologic diseases, metabolic and endocrine disturbances, transplantation and nephrology and models of health protection organizations. The United States and Poland signed an agreement last year for cooperation on general scientific research, and the National Science Foundation recently agreed to help finance the building of a centre for the study of astrophysics near Warsaw.

HEAO

Picking Up the Pieces

by our Washington Correspondent

DURING the past few weeks NASA officials have been quietly picking up some of the pieces of the High Energy Astronomy Observatory (HEAO) programme, which in January became a casualty of the Administration's drive to hold down federal expenditure. They have put together a plan to launch three small HEAO satellites with Atlas-Centaur rockets between 1977 and 1979, and to complete the programme with heavier satellites launched by the shuttle in the early 1980s. The emphasis in the first part of the revamped programme will be on X-ray studies.

The plan developed partly from a meeting, held shortly after the Office of Management and Budget killed the programme, of those who would have flown experiments on the original HEAO satellites. It has since been endorsed by the Space Science Board of the National Academy of Sciences. Although still rather tentative, the chief elements of the plan probably will not change much, but Dr John Naugle, Associate NASA Administrator for Space Science, said last week that the HEAO programme will remain suspended "for at least a year" while the details are settled.

As it now stands, the plan is for the first scaled-down HEAO satellite to do an X-ray survey, and for the second to study individual point sources of X-rays. The third satellite will carry gamma ray and cosmic ray detectors for a survey, and the shuttle-launched missions will carry the heavier gamma ray and cosmic ray experiments.

As originally planned, the HEAO programme would have consisted of two large satellites launched by Titan rockets in 1975 and 1977, followed by two more large satellites launched by the shuttle in the early 1980s. Each satellite would have carried a range of experiments to detect X-rays, gamma rays and cosmic rays, and each would have carried a scientific payload of about 12,000 pounds. In contrast, the Atlas-Centaur is capable of launching high energy astronomy satellites with a scientific payload of 2,800 pounds.

Why concentrate on X-ray studies for the scaled-down HEAO missions? Apart from the fact that most of the gamma ray and cosmic ray studies originally planned for the programme are too heavy to be launched by the Atlas-Centaur, it is felt that the results from the first Small Astronomy Satellite, Uhuru, which was launched in 1971, provide a good base on which to build. Moreover, the follow-up to Uhuru, SAS-C, which is now scheduled for launch in 1975, should provide even more exciting results as a curtain-raiser

to the scaled-down HEAO mission. As Dr Robert Halpern, Programme Manager of HEAO, said last week, "We know we have something to grab hold of, the other two fields have promise, but it's just that—promise".

X-ray astronomers are, understandably, relatively pleased by the plan to concentrate initially on their subject. Dr Herbert Friedman, of the Naval Research Laboratory, for example, said last week that although the revised programme is a "serious fallback" from the original plans, he strongly supports the plan to concentrate on X-ray studies. He pointed out that the HEAO experiment will be much more sensitive than Uhuru, perhaps even picking up several thousand sources compared with about 125 detected by Uhuru. The X-ray detectors on the reconstructed HEAO mission, which will be about 2 square metres, should also be capable of determining individual pulse

shapes from the Crab X-ray pulsar, and perhaps even of picking up the flashes of radiation that some theoreticians have predicted will be discovered coming from the area of black holes.

The new plan has not, of course, pleased everybody. Gamma and cosmic ray scientists are disturbed that if there are cost overruns on the first two missions, the third may be scrapped or at least delayed until the shuttle is available. Their fears may be justified. A year ago, Dr Naugle told the House Committee on Science and Astronautics that the costs of the first two original HEAO missions were then reckoned to be about \$180 million, and that even if possible development problems were taken into account, the total cost should be less than \$250 million. Last week, however, a NASA press release said that the cost of the original programme would have been about \$275 million.

ENERGY

Another Apollo

by our Washington Correspondent

AN energy research and development programme, akin to the Apollo or Manhattan projects, has been proposed by Senator Henry M. Jackson, the Washington Democrat who has effectively staked out the chief claim to energy matters on Capitol Hill. Backed by Senator Jennings Randolph, Chairman of the Senate Public Works Committee, Senator Warren Magnuson, Chairman of the Senate Commerce Committee, and some 25 other senators, Jackson has introduced a bill calling for expenditures of \$20,000 million by the federal government on energy research and development over the next ten years. The objective, Jackson said last week, is to make the United States self-sufficient in energy supplies by 1983.

Suggesting that present federal funds for energy research and development programmes are inadequate and illogically distributed, Jackson has proposed that they should be increased to \$1,607 million in the 1974 fiscal year, and that they should reach nearly \$2,000 million in 1977. This compares with \$772 million which the Administration has requested for next year, and which President Nixon proudly proclaimed in his recent message on natural resources.

Jackson's bill also proposes the setting up of an Energy Research Management Project, composed of officials from federal agencies involved with energy matters, to coordinate activities and to fund projects.

The share of the research and development effort devoted to non-

nuclear energy would also be substantially increased by Jackson's bill. It calls for the setting up of five corporations, jointly managed by the federal government and by industry, whose business would be to bring new technologies to the point of commercial application. Specifically, the corporations would be involved with coal gasification, shale oil development, advanced power cycles, geothermal resources and coal liquefaction, and the federal share of the effort would range from 50 per cent in the case of shale oil, to 80 per cent for the development of geothermal energy.

What chance does the bill stand of being passed by Congress? For one thing, it has strong bipartisan support, including the chairmen of the three committees most involved, and for another, as Jackson said last week, it will be considered later this year, when petrol rationing may become a reality in some parts of the United States. "I don't think we will have much trouble getting it passed," he said.

Whether President Nixon will agree to it is, however, another matter. He is now expected to produce his own energy message in late April, but his proposals will certainly be much less extravagant than Jackson's. Congress may have the votes to override a possible Presidential veto, but the Administration would probably not spend all the money even if the bill were rammed through. If the bill is passed in its present form, however, President Nixon would be forced by law to put into effect the institutional arrangements it entails, regardless of whatever he proposes in his own energy message. Jackson has thus neatly scooped the White House.

NEWS AND VIEWS

A New Step in Excitation-Contraction Coupling

IN striated muscle fibres the mechanical response follows the electrical activity of the outer membrane after a very short delay of a few milliseconds. The mechanism connecting the two phenomena has been referred to as excitation-contraction coupling (E-C coupling), an expression coined by Sandoz in the early nineteen-fifties. At that time virtually nothing was known about this coupling, except that A. V. Hill had calculated that the diffusion of a hypothetical "activator", liberated by the action potential from the outer membrane, would be much too slow to account for the rapidity of muscle activation.

A great advance towards the understanding of this problem was made when A. F. Huxley and his colleagues provided evidence that the so-called transverse tubules (T tubules: more or less radial invaginations of the outer membrane occurring at every sarcomere) could propagate the excitatory signal to the centre of the fibre. The uncoupling between excitation and contraction produced by the selective disruption of these tubules added further support, and the first step of E-C coupling was thus solved.

The later steps are now also reasonably well understood: the use of intracellular indicators for calcium has clearly demonstrated that in response to the excitation, a massive and sudden increase of Ca^{2+} in the sarcoplasm precedes the onset of the mechanical activity (Jobsis and O'Connor, *Biochem. Biophys. Res. Commun.*, **25**, 246; 1966; Ashley and Ridgway (*Nature*, **219**, 1168; 1968). This calcium reacts with the troponin complex located on the thin filaments, relieves the inhibition imposed by this complex on the actin-myosin interaction, and contraction occurs.

But an important question remains to be solved. How does the electrical signal propagated along the T tubules produce this liberation of calcium? This question is particularly fascinating because of the structural relationships between the T tubules (vector of the excitation) and the longitudinal sarcoplasmic reticulum where enormous amounts of calcium are sequestered in the resting fibre. Electron microscopy has revealed that each T tubule is in contact with two lateral dilations of the sarcoplasmic reticulum forming a characteristic "triadic" structure which is repeated once or twice every sarcomere. Obviously the "triadic junction" is a key point of the electrical-calcium coupling.

On page 244 of this issue of *Nature*, Schneider and Chandler report a very interesting observation which seems directly relevant to this question. Using voltage-clamp techniques on muscle fibres, these authors discovered that imposed square pulse depolarizations produce a small outwards current, in conditions where all of the time and voltage-dependent changes in sodium and potassium currents are suppressed (using tetrodotoxin, tetraethylammonium and RbCl). This current decays with time. At the end of the pulse, an inwards current can be detected. The interesting point is that the total amount of charge involved in the "on" and "off" responses is identical and remains so in various experimental conditions.

Schneider and Chandler suggest that the simplest inter-

pretation of this relation is that fixed charges, located in or near a structure sensitive to the polarization, move reversibly in response to changes in polarization. This view is supported by the relation between the amount of charge which moves and the membrane potential. Practically no signal is detected when the holding potential is -100 mV or more negative. For less negative potentials, the amount of charge increases with depolarization following an S-shaped curve, with a mid-point around -50 mV and a maximum value around -20 mV. These figures are illuminating, for the movement of charge occurs in a range of potentials where the mechanical response of the muscle is critically dependent on the membrane polarization. This strongly suggests a connexion with the E-C coupling.

The nature of these charges and their location are unknown. Nevertheless, the speculation presented by the authors at the end of their article is very interesting because they interpret their observation in connexion with the fine structure of the triadic junction. Electron microscope studies of Franzini-Armstrong (*J. Cell. Biol.*, **47**, 488; 1970) have revealed that the mode of contact of the T tubules with the sarcoplasmic reticulum is unique among the various types of membrane contacts. The two structures are connected through dense projections ("feet") coming from the terminal cisternae of the sarcoplasmic reticulum, and the space between these feet is in free communication with the sarcoplasm. The density of feet is about 700 per μm^2 of tubule membrane. By comparison, Schneider and Chandler estimate from their results that the density of charge groups is around 300 per μm^2 , assuming that they are uniformly distributed on the outer and the T tubule membranes. Though the agreement of these densities might be quite fortuitous, it provides the highly stimulating hypothesis that the excitatory signal is passing from the T tubule to the sarcoplasmic reticulum membrane by way of charges moving in the connecting feet, and Schneider and Chandler have found how to follow the trace of this passage. If true, this is progress.

It remains to be solved how this possible charge transfer across the feet triggers the release of calcium. Schneider and Chandler cautiously suggest that the release is somehow a function of the quantity of charge involved, but the speculation could be pushed a little further. It is probable that the massive and sudden release of calcium produced by the excitation is caused by a permeability change of a large fraction of the sarcoplasmic reticulum membrane (or at least of the terminal cisternae). But the measurements of Franzini-Armstrong have shown that only a few per cent of the sarcoplasmic reticulum membrane are involved in the feet connexion. Therefore, the signal passing through the feet should somehow be "propagated" or "amplified" in order to influence large surfaces of the sarcoplasmic reticulum. In this context, it would be very interesting to see whether a suitable mechanism could be provided by the property of autoamplification of the release of calcium itself, discovered by Ford and Podolsky (*Science*, **167**, 58; 1970) and by Endo (*Nature*, **228**, 34; 1970) and his colleagues.

J.-M. G.

Replacing the Pancreas

THE introduction in 1921 of insulin into therapeutics was a remarkable advance and saved the lives of countless diabetics. Injecting insulin once or twice a day under the skin, however, is by no means the same as the beautifully controlled release of insulin from the beta cell into the pancreatic vein which, together with other homeostatic mechanisms, severely limits the variation in the levels of blood glucose in the normal individual. Even the best controlled insulin-requiring diabetic experiences swings of blood glucose well beyond the normal. In the fifty years since Banting and Best, improvements in the therapy of the insulin-requiring diabetic have been relatively trivial, though the wide range of insulin preparations currently available, with various properties and formulations, certainly makes it easier to find a treatment to match the individual patient.

Attempts are being made to mimic the natural pancreatic homeostasis by developing small, implantable glucose sensors which might be connected to an implanted source of exogenous insulin or to some alarm system, warning of excessive hyper- or hypo-glycaemia. J. S. Soeldner has described such a sensor (*Diabetes Outlook*, **8**, 3; 1973) which is about the size of a postage stamp and which, when implanted in the subcutaneous tissue of a rhesus monkey, responds to changing levels of blood glucose. Another sensor has been developed by Bessman and Schultz (*Horm. Metab. Res.*, **4**, 413; 1972). These are early days, however, and many technological problems must be overcome before this apparatus becomes an alternative to the pancreatic beta cell. Nevertheless, it is a promising line of research and one in which technology, rather than biology, is the obstacle.

Biology is the major obstacle to another approach—pancreatic transplantation. Experimental heterotopic transplantation has been studied in the dog, but the longest survival has been only 139 days, even with an intensive regime of immunosuppression (Idezuki *et al.*, *Surg. Gynec. Obstet.*, **126**, 1002; 1968). In man, most operations have combined kidney and pancreas grafts, the surgery being performed on diabetic patients who had renal failure, in whom prognosis for life was poor. To date twenty-three pancreatic transplants have been reported (*J. Amer. Med. Ass.*, **217**, 1520; 1971), the longest survival being one year—better than the dog, but not a very encouraging record. Although the grafts functioned, survival was too short for any adequate assessment of continuing endocrine function by the grafts. But even if the problems of rejection are overcome and results improve dramatically, simple logistics rule out a significant contribution from pancreatic transplantation to the cure, or amelioration, of diabetes.

A possible alternative may be the transplantation of the islets of Langerhans themselves. In recent years a number of techniques have been developed for isolating the islets for various kinds of investigation. In terms of yield, the technique initially described by Moskalowski (*Gen. Comp. Endocr.*, **5**, 342; 1965), using collagenase to free the islets from other pancreatic tissues, has proved most useful. A modification of this technique is described by Thomas and his colleagues on page 258 of this issue of *Nature*. Moskalowski (in *The Structure and Meta-*

bolism of the Pancreatic Islets, edit. by S. Falkmer, B. Hellman and I. B. Täljedal, Pergamon Press, 1970, p. 73) transplanted guinea-pig islets into various sites in other guinea-pigs and found that they retained the ability to produce and store granules for at least 14 days. Strautz (*Diabetologia*, **6**, 306; 1970) and later Gates *et al.* (*Lancet*, ii, 567; 1972) transplanted into genetically-obese mice islets from their lean litter mates. These islets were contained in 'Millipore' diffusion chambers and were implanted intraperitoneally. Not only did the islets survive for at least 10 weeks, but the implanted mice reverted to normal weight and normal levels of glucose tolerance and plasma insulin. Ballinger (*Diabetes Outlook*, **8**, 3; 1973) has reported improvements in the metabolic state of rats made diabetic by streptozotocin injections when islets from normal rats were implanted.

Although many problems remain to be solved before islet transplants can make a significant contribution to the treatment of human diabetes, it seems a more promising line of research than transplantation of the whole pancreas.

R. J. J.

Mediterranean Evaporites

THE widespread occurrence of an evaporitic suite in the Mediterranean has been confirmed by the Deep Sea Drilling Project cruise leg 13. The presence of evaporites in the western sea had been suspected from seismic reflexion profiles (Hersey, *Bull. Geol. Soc. Amer.*, **76**, 1251; 1965) but their age remained debatable until the drilling proved that they are Upper Miocene. The evidence from leg 13 creates more controversy, however, in this case about the environment and mode of their formation.

Three possible models could account for the origin of the evaporites. In the first, a deep basin, renewed by water from the Atlantic, maintains its level during evaporation at the world-wide sea level; this is the "deep water, deep basin model". In the "shallow water, shallow basin model", connexion with the Atlantic permits the brine level of a shallow basin to remain at world-wide sea level. A third "desiccated deep-sea basin model" allows the evaporation of a series of salt lakes or playas, which lay several thousand metres below the sea level of the Atlantic.

The evaporitic suite obtained from drilling during leg 13 comprises halite, anhydrite, gypsum and dolomite. All available evidence from mineralogical, petrographical, sedimentological and geochemical investigations indicates that these evaporites were in fact formed in shallow water. The presence of anhydrite in nodular and "chicken-wire" form strongly suggests a subaerial environment of origin, similar to present day Sabkhas of the Persian Gulf.

Cyclic deposits, discovered in a hole to the south of Mallorca, reveal sequences, suggestive of deeper to shallow-water formation, which consist of laminated dolomites through to stromatolites; these indicate that periods of inundation successively follow periods of desiccation. The scarce fauna and flora contained in the carbonate sediments are of brackish water association, whereas the halite beds are barren. The latter also show signs of desiccation and replacement by anhydrite crystals, possibly during subaerial erosion. It is apparent also from marine inter-

calations within the evaporites that intermittent flooding of a desiccated basin has occurred.

A further indicator of a shallow water environment is the distribution pattern of the evaporites. Typically, playa deposition constitutes a "bull's eye" zonation of concentric zones of salts, with the inner zone the most soluble, that is halite in this series. This seems to be the case, for halite coincides with the areas of deepest water in the western basin, whereas it was not found in the east. A restricted basinal type of deposition would require a "tear drop" pattern of deposition, where less soluble salts occur near the opening and soluble salts at the far end. With the limited coverage attained by JOIDES sites, however, it is difficult to speak authoritatively on this matter.

The greatest disagreement emerges during discussion of the depth of the basin itself, the salient problem being whether the water level altered intermittently or whether the bottom of the basin moved up and down catastrophically to produce the great thicknesses of evaporites attained in the Mediterranean. Hsü *et al.* (*DSDP Initial Reports*, II, 43, 1203; 1973; and page 240 of this issue of *Nature*) favour the existence of a deep depositional basin largely on the strength of palaeontological evidence. The sediments immediately overlying the evaporites contain deep-water foraminifera of Pliocene age, whose appearance would be difficult to envisage in the event of an inundation of a shallow basin of Upper Miocene age. Also the sediments of Middle Miocene age cored from the Hellenic Trench are of deep water pelagics so that it seems probable that the basin had a generally deep base level. Geomorphological evidence has been recognized by Hsü *et al.* from adjacent land areas, where channels, cut by streams rejuvenated during regression of the sea, have been infilled by alluvial and terrestrial clastics. Steeply graded canyons are apparent at the continental margins, and these are probably drowned river valleys of Upper Miocene age. Nesteroff (*DSDP Initial Reports*, II, 21, 673; 1973), however, describes these canyons as a rejuvenated Middle Miocene system which was recut during subaerial erosion in a shallow basin. This is one example of a series of data which may be adapted to fit either hypothesis.

Finally, Nesteroff uses the evidence of local vertical tectonic activity of post-Miocene age in the Mediterranean area to argue for a shallow basin formation. His model requires the existence in the Serravallian of several shallow basins, of restricted circulation and connexion, which became completely cut off from the Atlantic during the Messinian. Desiccation followed, and was relieved only by periodic influxes from the Straits of Gibraltar. These permitted the accumulation of great thicknesses of evaporites in a central trough and thinner peripheral deposits on the slopes of the basins. Then, during the Pliocene transgression,

deep pelagic oozes were deposited and vertical movements led to the subsidence of the basins and uplift of the surrounding margins.

On the other hand, there is Hsü's picture of a deep hole, intermittently refilled during the Messinian by overflow from the Straits of Gibraltar, the brine content in the basins being successively replenished. The progress is recorded by the interbedded marine marls of the Upper Miocene evaporite formation of Sicily. The facts are inadequate to prove either hypothesis fully; neither will be credible until further drilling reveals the detailed sequence of pre-evaporite sediments.

From a Correspondent

Unique Sequences in Eukaryotic mRNA

IN next Wednesday's *Nature New Biology* (March 28) Dina, Crippa and Beccari describe experiments designed to determine whether or not there are, in a population of mRNAs from developing *Xenopus* embryos, sequences transcribed from reiterated DNA.

Labelled polysomal mRNA extracted from dissociated *Xenopus* embryos was found to have a high specific activity and was shown to be free from contamination by labelled ribosomal sequences. These features, combined with the use of stringent hybridization conditions to prevent degradation of RNA during the annealing reaction, provided the basic requirements for successful RNA hybridization with DNA in excess.

When *Xenopus* mRNA was annealed to DNA, only 5 per cent of the input RNA hybridized before a C_0t of 10^{-1} . At C_0t 100, 13 per cent had hybridized and the rest of the hybridization followed kinetics similar to those of unique sequences reaching a final value of 61 per cent between C_0t 5×10^3 and 2×10^4 . In ideal conditions for DNA excess hybridization, however, up to 90 per cent of input RNA should be hybridized.

Using a filter trapping method mRNA reassociated with DNA starting with a very low C_0t 10^{-3} and at C_0t 100 the reaction was complete when 80 per cent of the input RNA was retained by filters. The hybridization curve revealed two main components renaturing at different rates with C_0t $1/2$ of 5×10^{-2} and C_0t $1/2$ 20 to 30. Only a small percentage of the RNA bound to filters, however, was RNase resistant.

The melting profiles of DNA/mRNA hybrids obtained at low (100) and high (3,000) C_0t values are practically identical; they are very sharp and have T_m s very close to the T_m of native *Xenopus* DNA. These facts suggest that the hybrids which are formed are very

specific, and, because they are so similar to the melting curve of DNA, there must be some mRNA complementary to rapidly renaturing reiterated sequences.

Dina *et al.* suggest that these results are best explained if one supposes that each mRNA molecule consists of a part which hybridizes to repeated sequences and a second part which is complementary to a unique sequence. Thus, in the filter trapping experiment, the repetitive part of the mRNA molecule hybridizes rapidly to reiterated DNA; the rest of the molecule is also retained by the filter and complete (80 per cent) retention is obtained at a low C_0t 100. They also suggest that the long incubation times required for high C_0t values lead to thermal breakage of the partially hybridized mRNA. The RNA fragments broken off would contain unique sequences which would then hybridize more slowly, giving rise to the 61 per cent hybrid obtained at the end of the annealing reaction.

If each mRNA molecule consists of a fast and a slow annealing part then partial degradation of the mRNA molecules should shift the kinetics of reannealing. When alkaline digested mRNA was hybridized to DNA the kinetics were similar to the reaction with intact mRNA up to a C_0t 50. But in this case hybrid formation reached a final value of 90 per cent of input RNA at a C_0t of 2×10^4 .

These results indicate that each mRNA molecule contains a main part, which is transcribed from unique DNA, and a covalently linked small part transcribed from a family of homogeneously repeated sequences. If repeated sequences are bound to many different unique sequences they should be found dispersed throughout the genome. Indeed when DNA was fractionated on CsCl_2 the mRNAs hybridized to main band DNA and not to satellite DNA.

RABIES

Problems of Control

from our Medical Virology Correspondent

RABIES is not declining; on the contrary it is spreading throughout the world especially among wild animals. Rabies virus is classified as a member of the rhabdovirus group, which contains a diverse collection of bullet-shaped RNA viruses including vesicular stomatitis virus with a bovine host, Egtved virus of rainbow trout, Marburg virus (see *Nature*, **233**, 236; 1971), Sigma virus of *Drosophila*, Flanders-Hart Park virus with a mosquito host, potato yellow dwarf virus, maize mosaic virus and others.

Rabies virus is transmitted by contact, usually through a bite; there are no known vectors. It spreads from the site of infection along the peripheral nerves towards the central nervous system through nerve-associated tissue spaces, although this is by no means certain. Similarly, there is no explanation for the dysfunction caused by the virus in the central nervous system because only minimal cytotoxic effects are usually observed in cells infected with rabies, but the host's immune response may contribute to the pathological effect.

Rabies is enzootic in all the continents except Australia and Antarctica. Many of the large islands, however, such as Britain, Cyprus, Hawaii and New Zealand are free of the disease largely because of rigidly enforced quarantine regulations concerning the entry of dogs, cats and other animals. From the standpoint of human infection, the dog has remained since ancient times the most important reservoir and transmitter of rabies. Wildlife rabies, on the other hand, constitutes the greatest problem in many areas; knowledge of the ecology of wildlife rabies, however, is fragmentary.

In Asia the pariah dog is the principal carrier of the disease, although jackals are frequently involved. Mongooses and meerkats are important rabies reservoirs in South Africa and vampire bats in Central and South America. In the United States the skunk is the most important transmitter in certain areas and bats in other regions. Sledge dogs and silver foxes are mostly affected in the Arctic region. Badgers often live in close proximity to foxes and have been frequently infected in the current epizootic of fox rabies in central Europe (M. H. Kaplan, *Nature*, **221**, 421; 1969).

The present epizootic of fox rabies in Europe is particularly disquieting. The problem is best illustrated by the following incidents. At the end of last December a 20-year-old American soldier reported to a hospital in Saigon with clinical signs suggestive of rabies. He died two weeks later. This man had

played with a stray puppy on November 14 and was slightly bitten on the hand. Another serviceman also bitten by the same dog reported the incident. The dog was confined and died 3 days later of rabies. The second soldier, however, was treated successfully (Communicable Disease Centre, *Zoonosis Surveillance*, February 1973). In another US Air Force base in Vietnam a puppy was adopted in the officers' quarters to be registered as a mascot. The puppy was involved in a fight, was scratched on the face and ultimately developed rabies. Thirty-one persons, in the meantime, were possibly exposed to infection at a party held in the officers' quarters and rabies prophylaxis, unpleasant at the best of times involving up to twenty-one inoculations and not infrequently with serious side-effects, was instituted for twenty-three persons (R. Moser, *Aerospace Medicine*, **43**, 778; 1972). These two incidents bear an important message for Britain because the principal risk of introduction of rabies to the country is by the irresponsible smuggling of dogs and cats; with the belt of rabies advancing in Western Europe this is a real hazard.

Human rabies prophylaxis is based chiefly on two types of vaccine (S. A. Plotkin and H. F. Clark, *J. Infect. Dis.*, **123**, 227; 1971). The efficacy of the nervous tissue vaccines, Semple and Fermi, has not been evaluated by controlled studies. The duck embryo vaccine has been favoured because of the lower risk of allergic encephalitis, although better antibody responses are invoked by the Semple vaccine. There are also several experimental vaccines. The Flury-HEP vaccine is a live virus passaged 180 times in chick embryos, but it elicits a poor antibody response

and it is not suitable for use after exposure. Semple-type vaccines using suckling mouse brain are available in some countries. More promising are rabies vaccines prepared in tissue cultures which completely lack nervous tissue and, therefore, are not expected to be encephalogenic. The outlook for man is thus hopeful and indeed currently between 700,000 to 1,000,000 persons are subjected annually to a rabies immunization regime; but the problem of controlling wildlife rabies remains an impossible task.

NEUROPHARMACOLOGY

Opiate Receptor Binding

from a Correspondent

WITH federal support of basic research plummeting, bioscientists in the United States are hastily reviewing their research for evidence of social significance. One group that will not have far to look is Snyder's team at Johns Hopkins University Medical School. In a recent issue of *Science* (**179**, 1011; 1973) Pert and Snyder announce their identification in mammalian nervous tissue of a receptor which specifically binds opiates.

Binding was demonstrated by incubating tissue homogenates with tritium-labelled naloxone, a potent antagonist which precipitates withdrawal in humans addicted to opiates. Naloxone, rather than an agonist such as morphine, was chosen on the premise that as an antagonist its binding would be less reversible, and thus the drug-receptor complex might hang together long enough to be measured. After incubation, the homogenates were washed through a filter which trapped large

Lepore and Anti-Lepore Haemoglobins

THE "Lepore" and "anti-Lepore" haemoglobins are of great theoretical interest because they illustrate at a molecular level the consequences of the process of gene crossing-over. The non- α chain of Hb Lepore-Hollandia, the first example of this type of haemoglobin variant to be discovered, was found to contain N-terminal and C-terminal regions derived from normal δ and β chains respectively. In 1964 Smithies showed how such a chain could arise from homologous but unequal crossing-over of the δ and β chain genes. Haemoglobins Lepore-Boston and Lepore-Baltimore differ in the relative lengths of the δ and β segments of the δ - β chain.

In *Nature New Biology* next Wednesday (March 28) Badr, Lorkin and Lehmann now report that the non- α chain of Hb P-Nilotic is a reversed or anti-Lepore β - δ chain, with the N-terminal

region derived from the β chain and the cross-over occurring between positions 22 and 50. This clearly distinguishes it from Hb P-Galveston (β -117, G19, His $\rightarrow$ Arg) which has the same electrophoretic mobility.

Purified Hb P-Nilotic has a slightly higher oxygen affinity than Hb-A, but the haem-haem interaction and Bohr effect are very nearly identical. It is the second anti-Lepore haemoglobin to be described. The other is Hb-Miyada, in which the β - δ cross-over occurs before position 22, so that its electrophoretic mobility is the same as that of Hb-A₂.

As Lehmann and his colleagues point out, it will be of great interest to study the biosynthesis of the β - δ chains of Hb P-Nilotic to ascertain if this occurs only in the earlier stages of erythroid cell maturation, as is the case for the δ chain of Hb-A₂ and the δ - β chain of Hb-Lepore.

molecules such as receptors but let through the smaller, unbound naloxone molecules. The filters were then counted to determine the amount of radioactivity bound to them.

It was necessary to demonstrate that the tritiated naloxone trapped on the filters was specifically bound to receptor molecules and not just stuck unspecifically to macromolecules. This was achieved by adding opiate agonists such as levorphanol, morphine, or methadone to the incubation mixture. The analgesic effect of opiates *in vivo* is very stereospecific; activity is almost completely limited to isomers orientated analogously to D(dextro-rotary)-morphine. Thus, if the physiological response parallels receptor affinity, D isomers of the opiates should bind much more readily than their L isomers. This was found to be the case: low concentrations of D-isomer opiates drastically reduced naloxone binding (by competing for the receptor sites), whereas the L-isomers were without effect. In this fashion 75 per cent of the radioactive naloxone trapped on the filters was determined to be bound stereospecifically. Furthermore, the affinities of the various opiates for the receptor corresponded to their relative potencies for producing analgesia.

Further evidence for specificity of binding was seen by the fact that specific binding was maximal at physiological temperature (37° C) and almost nonexistent at 4° C. Specific binding was also pH dependent, peaking at pH 7.4.

In this investigation opiate receptor binding was found exclusively in neural tissue. Several investigators have used contraction of the guinea-pig small intestine as a physiological indicator of response to opiates. Pert and Snyder demonstrate that de-innervation of the intestine completely abolishes specific binding.

What is the function of the opiate receptor? It hardly seems likely that vertebrate nervous systems possess a sensitive receptor for a molecule that is not normally found in vertebrates. More likely, the opiates are acting as analogues of some endogenous molecule. Whatever the nature of this molecule, it is undoubtedly important and its elucidation may significantly advance understanding of neural function.

Even before the theoretical implications of the opiate receptor are worked out, this technique is likely to attract widespread attention. As Pert and Snyder laconically point out, their assay offers a simple means of rapidly scanning the relative effects of potential narcotic agonists and antagonists. This could prove a valuable tool for bio-scientists bent on finding a medical solution to the problem of drug abuse.

TRANSFER RNA

Clues on Conformations

from our Molecular Biology Correspondent

TRANSFER RNA needs magnesium to maintain its functional conformation, in a specific manner that is not to be equated with the mere stabilization of base pairing by increasing ionic strength. This observation led to the recognition of a relatively well-defined "denatured" state, with apparently marginally less base pairing than the native, but differing from it by a wide selection of criteria. Whether the denatured state of transfer RNA has any functional role in the cell is questionable, but it is of interest particularly for what it might tell one about the way in which the chain selects one among many possible alternative base-pairing schemes in a given set of conditions. The conformation of one, and as the crystallographers would undoubtedly maintain, the native form of a tRNA should be explicitly known soon,

and has already been adumbrated. It is not, however, clear that other conformational isomers can be crystallized, and recourse to indirect methods of evaluating the secondary structure, or at least how it differs from that of the native molecule, is therefore inescapable. So far the most convincing attempts at defining these differences have originated from experiments on the binding of oligonucleotides to available unpaired parts of the chain, and from examination of the sites of scission by nucleases.

A curious variation on this last approach has been evolved by Wintermayer and Zachau (*Biochem. Biophys. Acta*, **299**, 82; 1973) who have discovered an unsuspected specificity in metal-catalysed alkaline hydrolysis. With magnesium as catalyst, chain scission ensues at pH 9.5 and 50° C, and fragments progressively appear in electrophoretic gels. With yeast phenylalanine-tRNA, if the magnesium:phosphate ratio is greater than about 0.5, two fragments are formed with a high degree of

The Identity of Cytotoxic Cells

THE revolution in cellular immunology which led to the concept of two principal classes of lymphocytes, T cells of thymic origin and B cells originating from the bone marrow or a bursal equivalent, has passed its phase of turbulence. Emphasis is now changing, on the one hand, to clarification of the various descriptive characteristics of the two classes of cell and, on the other hand, to elucidation of their functional capabilities. Clearly in the long run these two avenues of approach should be confluent; at present they are not.

It has been argued in relation to anti-tumour immunity that T cells can be cytotoxic against malignant cells but that their aggressive effect may be inhibited by antibody which is a B-cell product. B cells, on the other hand, have apparently been shown to be cytotoxic but their killing potential requires an antibody-sensitized target cell. The complicating feature of the whole response pattern is that the production of both blocking and sensitizing antibody (possibly the same entities) may well require cooperation between T and B cells. In next Wednesday's *Nature New Biology* (March 28) Greenberg *et al.* suggest that B cell cytotoxicity is brought about by a class of B cell lacking detectable surface immunoglobulin.

Greenberg *et al.* investigated the capacity of normal (Balb/c) mouse spleen cells to lyse chicken erythrocytes *in vitro*. The target cells were previously sensitized by incubation in a rabbit anti-chicken erythrocyte antiserum. After depleting the spleen cells of macrophages by incubation with iron

carbonyl and of immunoglobulin-bearing (B) lymphocytes by passage through a 'Sephadex' column which had been coated with a heterologous anti-mouse Fab, the cytotoxic potential of the remaining cells was found to be greater than that of the starting cell population. Furthermore, when the B cells were released from the 'Sephadex' column by incubation with dextranase they exhibited no cytotoxicity.

Greenberg *et al.* then took the cell effluent from one of their anti-Fab coated columns (without dextranase treatment) and reacted it with a heterologous anti-T cell antibody in the presence of complement. This device reduced the number of viable cells by 82 per cent, presumably by killing the T cells present, but failed to alter the cytotoxicity against sensitized chicken erythrocytes. Along similar lines it was shown that the spleen cells of mice depleted of T cells by a schedule of neonatal thymectomy, irradiation and treatment with an anti-lymphocytic antiserum were usually cytotoxic in the test system.

The authors conclude from these experiments that there exist cells which are not T cells and which do not have immunoglobulin on their surfaces (a usually accepted characteristic of B cells) but which nevertheless can be cytotoxic against sensitized nucleated cells. They indicate the possibility that such cells are not lymphocytes. Thus the nice neat division of cytotoxic lymphocytes into T and B depending on whether they are blocked or potentiated by antibody may not necessarily be true.

specificity. From the size of these fragments, and the presence of dihydrouridine in both, it follows that the primary site of scission is between positions 16 and 17, that is to say, in the dihydrouridine loop. This remains true when the bulky Y base is first chemically excised from its position in the anticodon loop, and seems to extend equally to serine-tRNA. If the magnesium concentration is reduced below the critical value, catalytic efficiency is unimpaired, but a striking change occurs in the fragmentation pattern, with advent of new components, in particular a large piece of nearly seventy nucleotides. There is circumstantial evidence to identify this as residues 11-76.

If the conformation is further and more drastically weakened, by addition of 7 M urea, the hydrolysis becomes totally non-specific. The inference from these results is that the alkaline hydrolysis proceeds by way of a cyclic phosphate intermediate, which can only form when there is sufficient local chain flexibility. This condition is presumably fulfilled at the bond joining the non-aromatic nucleotides (which do not stack). The absence of hydrolysis at other sites in the chain points to a rigid structure, which is noteworthy particularly as regards the anticodon loop. At lower magnesium concentration at the working temperature, a transition to a state with more degrees of conformational freedom evidently occurs, and this is very likely a still highly base-paired denatured form.

The conformational difference between the native and denatured states is quantitatively reflected in the tritium-exchange experiments of Webb and Fresco (*J. Mol. Biol.*, **74**, 387; 1973). The kinetic profiles are strikingly different in the two forms, with much faster exchange in the denatured. This again is not simply an electrolyte effect, for with a leucine-tRNA, which has the property found in some but not all species, of a high activation barrier between the two conformations, so that it can be trapped in either at will in the same condition, the kinetic difference persists in its entirety. Evaluation of the total number of slowly exchanging hydrogens leads to a value of 111 for native leucine-tRNA and 95 for denatured. This indicates that a few more bases are paired in the native state, though the precise allocation of numbers of exchangeable protons to A-U and G-C base pairs (let alone any G-U pairs that may exist) involves a margin of uncertainty. The differences in exchange rates are thought to be related to tertiary structure, which is certainly more compact in the native molecule.

Webb and Fresco offer a further observation, which may bear on the mechanism of the conformational

transition. This has a high activation energy (of the order of 70 kcal mol⁻¹ for renaturation), which suggests that to permit the structural rearrangement momentary opening of one or more base-paired tracts must occur. If such a transitional state were to persist for a sufficient interval of time, it should manifest itself in a sudden release of exchangeable protons, and this is exactly what Webb and Fresco report. Introduction of magnesium into unfractionated tRNA at an early stage of exchange causes the kinetics to lurch abruptly onto a new smooth curve. Webb and Fresco are properly cautious, for the suggested interpretation would have the unpleasing implication that the barrier is actually diminished by magnesium, which would normally be expected to stabilize all base-paired states relative to the unfolded. Moreover, such a very long-lived intermediate should manifest itself in the relaxation kinetics of the transition, and this has not been observed.

That a significant difference in the extent of base pairing actually exists, as between the native and denatured leucine tRNA, is shown by Wong *et al.* (*ibid.*, 403), using high-resolution proton magnetic resonance spectroscopy in water solution, where downfield resonances arising from slowly exchanging protons involved in base pairing can be resolved. With the aid of assignments based on earlier work, three to five G-C pairs are shown to be lost on denaturation, and up to two A-U pairs possibly gained. Wong *et al.* note that in the leucine-tRNA the dihydrouracil

stem comprises only three G-C pairs, and is thus an obvious candidate for the site of the conformational difference. This would be consistent with the greater accessibility of this region to complementary oligonucleotides in the denatured molecule.

TRANSCRIPTION

New Termination Factor

from our Cell Biology Correspondent

A SMATTERING of Greek, or at least knowledge of the Greek alphabet, seems to be an essential requirement for anybody wishing to matriculate in the study of transcription by bacterial DNA-dependent RNA polymerase. Following the Hellenic inclinations of Travers and his associates, and Roberts, who selected sigma and rho to designate the factors which they discovered, Schäfer and Zillig have named as kappa a new termination factor, which blocks the transcription of phage T4, T5 and T7 DNA by *Escherichia coli* RNA polymerase (*Europ. J. Biochem.*, **33**, 201; 1973).

This new termination factor, kappa, was discovered when Schäfer and Zillig set out to isolate rho factor and is apparently a dimeric protein made of two identical subunits, each with a molecular weight of 17,000. It is present in *E. coli* cells at a concentration about the same as that of RNA polymerase; that is to say, each cell probably contains several thousand molecules of kappa. Kappa binds to DNA and does not inhibit initiation of tran-

Screen for Human Tumour Viruses

MURINE sarcoma virus (MSV) is defective; the virus transforms susceptible fibroblasts, but it cannot replicate infectious progeny in the cells that it transforms unless they are coinfecting by a murine or other mammalian leukaemia virus which complements the defect of the MSV genome.

Mouse cells transformed by MSV in the absence of a leukaemia virus helper are described as S⁺L⁻, and such cells can be used to test whether or not a stock of any virus contains helper activity for MSV because if the virus has such helper activity it should infect S⁺L⁻ cells and cause the release of infectious MSV. As Peebles, Fischinger, Bassin and Papageorge report in *Nature New Biology* next Wednesday (March 28), human amnionic cells can be transformed by certain strains of MSV to yield human S⁺L⁻ cells which should prove useful in the identification of putative human leukaemia viruses.

To induce MSV to transform cloned human amnionic cells Peebles *et al.* rescued MSV from mouse S⁺L⁻ cells

by superinfection with feline leukaemia virus. The rescued virus is a pseudotype, it has the MSV genome in a feline leukaemia virus envelope and is designated MSV(FeLV). This pseudotype virus, because it has a feline leukaemia virus envelope, can infect human cells and, as Peebles *et al.* found when human amnionic cells are infected with MSV (FeLV), clones of transformed human S⁺L⁻ cells can be isolated.

These human S⁺L⁻ cells do not release infectious sarcoma viruses, but if they are superinfected with feline leukaemia virus they begin to release MSV(FeLV) pseudotypes and if they are superinfected with RD114 virus, a candidate human C-type RNA tumour virus, they release a pseudotype MSV (RD114). Together with untransformed human amnionic cells, these human S⁺L⁻ cells may, therefore, as Peebles *et al.* conclude, "provide a unique *in vitro* method in a human system for the isolation, assay and identification of sarcomagenic and leukaemogenic RNA tumour viruses from human tumours".

scription, although it does reduce both the rate and the extent of transcription. In the presence of saturating amounts of kappa the average length of phage RNA transcripts is about 40–50 per cent that of transcripts made in its absence. Analysis by sucrose gradient centrifugation and by competition and saturation hybridization indicates that only a few of the kappa molecules bound to phage DNA halt transcription, and most of the bound kappa seems only to reduce the rate of transcription. Once transcription is halted by a kappa molecule bound at low ionic strength to specific kappa termination sites, the RNA transcript, the DNA template, the kappa molecule and polymerase remain associated in some quaternary complex and, according to Schäfer and Zillig (*ibid.*, 215), this is also the case when transcription is terminated by factor rho.

Having isolated kappa, Schäfer, Zillig and their associates (*ibid.*, 207 and 215) investigated the influence of factors and ions on the initiation as well as the termination of transcription by *E. coli* RNA polymerase. Their data on the rate and pattern of initiation of transcription of native T4, T5 and T7 DNA by RNA polymerase containing sigma factor, in conditions of low and high ionic strength, and in the presence and absence of heparin, led them to an interesting model of a possible sequence of events leading to initiation. Schäfer *et al.* propose that the promoter site on DNA is functionally differentiated into an entry site, R, to which the RNA polymerase molecules bind. They further postulate that the enzyme molecules are then shunted to a storage site, S, where they can become stably aligned and associated with a template before moving to the actual initiator site, I. Whether or not this model of a differentiated promoter can be verified remains to be seen.

And what about termination? Transcription of T7 and T5 DNA in the absence of factors is terminated in conditions of both high and low ionic strengths with the release of RNA, but polymerase is only released at high ionic strength. With T4 DNA as template and at lower ionic strength neither the enzyme nor the RNA transcript is released. At low ionic strength, in the presence of termination factors rho and kappa, transcription of T4, T7 and T5 DNAs is terminated without the release of enzyme or RNA transcript. Just how rho and kappa act *in vivo* and how they are counteracted has yet to be elucidated.

In the same issue of the *European Journal of Biochemistry* (*ibid.*, 301) Smith reports experiments probing another initiation event—the initiation and translation of EMC virus RNA in a cell free system from Krebs II ascites cells. This work not only supports the

argument that met-tRNA_{met}^{met} is the universal initiator of translation in eukaryotes, but also that the EMC RNA, which codes the nine proteins at least *in vivo*, has a single initiation site for translation and that the polypeptide made immediately after the initiation site is metabolically labile and not part of any of the nine EMC proteins.

SELENOLOGY

Moon Matters

from a Correspondent

THE First Lunar Science Conference in January 1970 was almost entirely devoted to reports of lunar sample investigations. Subsequent conferences have, however, broadened in context and the most recent conference, held from March 5 to 8 at the Johnson Space Center, has included accounts by both Apollo 16 and 17 astronauts, theoretical contributions both on the formation of the mascons and the origin of the Moon, Soviet analyses of material from Luna 16 and 20, and descriptions of experiments carried out on the lunar surface and from lunar orbiting spacecraft. It is also apparent that there is a big increase in the number of experiments aimed at laboratory simulation of lunar conditions and processes.

Many new interesting results concerning the immediate lunar surface layer were reported. An infrared radiometer (Professor F. J. Low, Rice University, and Dr W. W. Mendell, NASA) was used to scan the lunar surface from the Apollo 17 service module. The results show night-time surface temperatures below 90 K, indicating a considerably lower surface thermal conductivity than had been previously assumed. Dr H. Mizutani (Caltech, Pasadena) reported that the insulation of this layer may have appreciable effect on the thermal evolution of the Moon. Dr A. Dollfus (Meudon Observatory, Paris) described the extension of his work on optical lunar polarimetry which now reveals widely differing properties of the surfaces of the asteroids. The carbon in the lunar surface layer now seems to be entirely the result of absorption of that element from the solar wind. Dr G. Eglinton (University of Bristol) and others gave evidence to show that this carbon is converted to iron carbide during the melting process associated with meteoritic bombardment and is later distributed throughout the broken regolith layer by subsequent meteoritic churning of the surface. Mineralogical arguments were put forward to show that the glass globules found in the regolith have cooled at rapid rates (~ 100 K

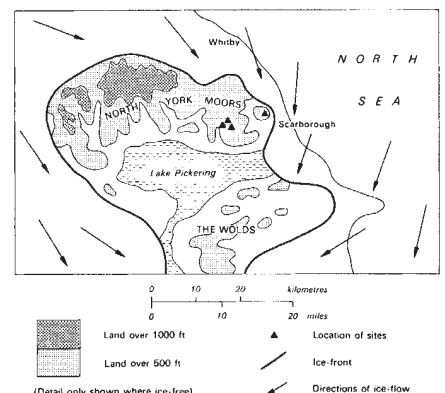
Red Palaeosols Found in Northern England

RED palaeosol features found in sandstones and shales in the north of England provide a clue to the climate of the area in Pleistocene times. In next Monday's *Nature Physical Science* (March 26) Bullock, Carroll and Jarvis describe three such palaeosols which they have found, and a fourth reported by Curtis. All four are in Yorkshire (see map); red mottles occur in clays 13 km north-west of Scarborough, sandstone blocks with a red rind are found in the Langdale Forest 16 km north-east of Pickering, brown and yellowish-brown sandy clay loam and occasional red mottles 10 km west of Scarborough, and a "white material with many ferruginous concretions" below a thin iron pan 10 km north of Pickering.

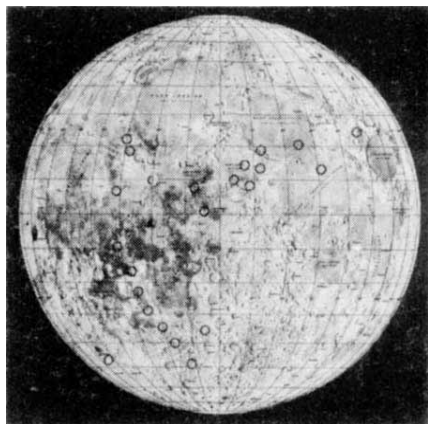
What is the significance of these features? According to Bullock and his colleagues, they are all similar to features commonly reported in regions of the tropics and sub-tropics which have marked wet and dry seasons. In Northern Nigeria, for example, all four kinds are found in a single landscape, as seems to be the case in Yorkshire. It seems reasonable to conclude that during the Pleistocene, when these deposits were formed, the climate of the region—and presumably of the rest of Britain—was of that kind. But the red mottles in shale and red rinds and clayey

weathering in sandstone only seem to be present in a 50 (km)² area, although the rock types are common in England.

It may be, argue Bullock *et al.*, that widespread glaciation has denuded large areas, removing similar features in other parts of the country. This could explain why these particular palaeosols are now found only on plateaux, protected by escarpments and sea cliffs. This immediately suggests suitable regions where similar features can be expected, and Bullock *et al.* point specifically to small plateaux at the edge of the Pennines as strong candidates, north and east of the presently known sites.



Red palaeosol sites and patterns of ice movement in the late Devonian, in East Yorkshire.



The near side of the Moon showing the epicentres of moonquakes recorded by lunar-based seismometers.

s^{-1}) in free flight above the lunar surface (Dr W. von Engelhardt, University of Tübingen). The orange soil near the Apollo 17 site was shown to consist chiefly of coloured glass. The most striking region, located in a cylinder 1 m in diameter and 0.3 m deep, showed no evidence of recent volcanic activity, but was formed nearly 4×10^9 yr ago and has been exposed near the surface for about 3×10^7 yr.

The installation of a network of lunar-based seismometers has led to some of the most impressive results of the whole Apollo programme. The outer 20 to 30 km of the Moon has been shown to be broken or fractured so that the propagation of seismic energy within this layer may be regarded as a diffusion process. The seismometers have also detected the impact of many large meteorites on the lunar surface, measuring both the energy and position of the impact. The statistics of such observations are in good agreement with other (often less reliable) estimates of meteorite flux.

Internal centres of lunar seismic activity have been located principally between 800 and 1,100 km below the surface (lunar radius 1,738 km). At the conference Dr G. Latham (University of Texas, Galveston) reported that the epicentres for such events (moonquakes) lie close to two great circles (see Figure). He also discussed the impact of a large meteorite near the Sea of Moscow on the lunar far side. The shear waves from this impact were completely suppressed by the central zone of the lunar interior and this may imply central temperatures as high as 1,800 K—a value many hundreds of degrees above that inferred earlier from observations of the response of the Moon to the electromagnetic field associated with the solar wind.

The sites chosen for the Apollo landings have moved progressively from the flat dark maria regions to lighter more rugged upland areas. The last two landings, in particular, have investigated

regions with kilometre scale mountains, large boulders many metres in size and even outcrops of rock. Subsequent terrestrial analysis of these rocks has shown that very few of the samples are similar to the Apollo 11 and 12 basalts, formed in the maria by solidification of large volumes of magma or lava. Instead these upland rocks are breccias, that is composite rocks formed by the compaction of powdered or broken rock under the influence of temperature or pressure. Breccias were also returned from the maria but the upland breccias are composed of particles of comparatively primitive anorthitic rock. These breccias range from fragile low-density samples to dense rocks which have undergone heat treatment in the lunar surface layer at temperatures well above 1,000 K and in some cases almost sufficient to return them to molten magma. This history has been confirmed by magnetic measurements reported at the meeting which indicate cooling from temperatures above the Curie point in the presence of magnetic fields estimated in the range 10^{-4} to 10^{-6} tesla (1 to 10^{-2} oersted). Still higher fields are required by magnetometer measurements carried out on the lunar surface. A dynamo mechanism in a once liquid lunar core could possibly produce such a field, but a more attractive possibility may be an early intense solar field invoked in modern condensation and accretion models for the origin of the Solar System.

Whatever the origin of this early magnetic field its discussion at this conference has demonstrated clearly that lunar science is not simply an isolated study. The antiquity of the lunar surface on a cosmological time scale makes it ideal

for experiments concerning changes in the rate of solar flare activity, the flux of meteorites and the cosmic-ray flux. Rarely regarded as intellectually respectable, erratically if lavishly supported by a few countries, lunar science has now returned to the stage where its adherents are chiefly motivated by scientific interest.

GLOMAR CHALLENGER

Red Sea Drilling

from our Geomagnetism Correspondent

IN view of the importance of the Red Sea as a region of apparently young seafloor spreading, the results of the visit to the area last year by the deep-sea drilling vessel *Glomar Challenger* are awaited with some anticipation. Full details will presumably become available slowly over the coming months and years; but in the meantime Ross *et al.* (*Science*, 179, 377; 1973) have reported preliminary results and conclusions derived on board during the voyage.

Three of the six holes drilled (225 to 230) were in the vicinity of the well-known hot brine area comprising the Atlantis II, Chain and Discovery Deepes where waters of high salinity and temperature are underlain by sediments enriched in heavy metals such as copper, lead, zinc, silver and gold. The core from site 225, which lies 16 km east of the Atlantis II Deep, indicated 177 m of late Miocene to late Pleistocene silt and ooze overlying 53 m of anhydrite and halite. The surface of these evaporites was found to correlate with the acoustic reflector S (velocity 4.4 km s^{-1}

Amino-acids in Lunar Samples

IN next Monday's *Nature Physical Science* (March 26) Modzeleski *et al.* report their analyses of seven samples brought back from the Moon by the Apollo 14 astronauts for carbon compounds and amino-acids. They used a combined vacuum pyrolysis-mass spectrometry technique for estimating gases containing carbon and hydrocarbons and ion exchange chromatography for quantifying the amino-acids. These latest results are part of an ongoing attempt to elucidate the history and manner of formation of these molecules on the Moon.

The principal gaseous products of the pyrolysis procedure were CO , CO_2 and CH_4 , and the results of pyrolysing 50 mg portions of the lunar samples indicated that carbon was present in the gases to the extent of between 76 and 161 parts per million. The largest relative amount of CO and CO_2 was given off by a breccia sample rather than by a sample of fines.

When the mass spectrometer was set

to record compounds with a larger molecular weight, many species came to light, for example C_2H_5 , C_3H_5 and COS . And sulphur was clearly evident as a ring above the cold trap of the pyrolysis tube. Modzeleski *et al.* say that their findings suggest that the sulphur was not present in elemental form nor in an organic compound but probably in a mineral in view of the high temperature needed to liberate it.

The ion exchange column used to analyse aqueous extracts of the lunar samples was capable of detecting as little as 5×10^{-11} mol of amino-acid. As it turned out, all the amino-acids detected were present in quantities less than 8×10^{-11} mol per gram of extract, except for glycine. Glycine, aspartic acid, glutamic acid and serine were the most abundant of the amino-acids detected. Tests on parts of the surface of one of the astronaut's gloves revealed no amino-acids, indicating that those detected did in fact come from the Moon.

compared with 1.6 to 1.7 km s⁻¹ for the overlying sediments) which has been mapped in many other regions of the Red Sea. But an even more interesting correlation can be made outside the area. Ross and his colleagues find that the Red Sea evaporites, dated by fossils in the interbedded shales, are of the same age as those previously discovered by deep drilling in the Mediterranean. They thus equate the age of the Red Sea reflector S with that of the Mediterranean reflector M.

The correlation of the evaporite surface with the widespread reflector S, backed up by direct access by drilling at other sites, implies that most of the Red Sea is underlain by late Miocene evaporites, the chief exception being the central rift valley. Ross *et al.* also find that the depth of the reflector S coincides with the depth of the brine interface in the Atlantis II Deep. This leads them to suggest that an occurrence of brine is possible wherever the evaporite outcrops in the vicinity of an enclosed basin and thus to predict the presence of other brine pools in the Red Sea. At the time that Ross and his colleagues were preparing their report, evidence for brine pools outside the Atlantis II area was scanty; but before the report had appeared, Backer and Schoell (*Nature Physical Science*, **240**, 153; 1972) had already reported the discovery of no less than thirteen new brine pools.

The widespread occurrence of late Miocene evaporites in the Red Sea also has important implications for seafloor spreading in the area. The evaporites are apparently absent from the central rift valley, but the new drill hole at site 227 showed that rocks as old as or older than Miocene occur within 1 to 3 km of what is thought to be an actively spreading ridge. This would seem to destroy the idea that spreading in the Red Sea has been continuous since a supposed beginning during the Tertiary. The most reasonable conclusion based on the data from site 227 is that the present phase of spreading began less than two million years ago following a hiatus which may have lasted since the early Miocene.

The other principal Red Sea theme is, of course, the occurrence of mineral deposits within the sediments. At site 225, dark shales were found to contain over 5 per cent iron, about 1,000 p.p.m. vanadium and about 500 p.p.m. molybdenum. Thin shale layers within the evaporite also contained 200 p.p.m. copper. Drilling at site 226 in the Atlantis II Deep was not entirely successful because of the presence of a shallow impenetrable layer of basalt. These basalts were mineralogically similar to deep oceanic tholeiites. Their metal contents were not significantly different from those of other basalts; but 14 m of sediments enriched in heavy

metals were recovered from above the basalt layer, and these may continue beneath. Site 227 (about 3 km east of the end of the Atlantis II Deep) also produced shale layers rich in vanadium and molybdenum, and copper concentrations were as high as 700 p.p.m. Ross and his colleagues suggest that these sediments could be the source of the metals in the hot brine deposits.

Site 228, near the axial valley but well away to the south-east of the Atlantis II Deep, gave sediment sequences similar to those at 225 and 227. Anhydrite was reached at a depth of 280 m but problems led to the abandonment of the hole before halite was reached. Ross *et al.* conclude, however, that halite lies within a few tens of metres from the bottom of hole 228. At sites 225 and 227, the salinity of the interstitial waters in the sediment was found to increase on approach to the evaporites to a value fairly close to that of a saturated sodium chloride solution. The assumption that a similar phenomenon applies in the vicinity of site 228 allowed the prediction of the depth of the halite. But perhaps of greater importance was the discovery of black shales containing high concentrations of zinc in the range 0.5 to 5.0 per cent. Thus in the Red Sea there are at least two suites of metalliferous shales—one enriched in vanadium and molybdenum and the other in zinc.

Movement of Atmospheric Carbon Dioxide

THE variations in carbon dioxide levels in the atmosphere with the changing season is a subject of wide interest. Until recently, however, few data were available for the Southern Hemisphere; but in 1972 the CSIRO Division of Atmospheric Physics began a monitoring programme based on the collection of samples by aircraft. In next Monday's *Nature Physical Science* (March 26) Garratt and Pearman present the first results of this programme, which cover the period May 1972 to February 1973.

Most of the samples were collected by aircraft over the sea to the south-east of Australia, and these include samples collected on the same day (or within a few days) from two or more levels in the middle and upper troposphere. From June to September, there was an average increase of CO₂ content with height of 0.4 p.p.m. between 4 and 10 km, suggesting downward transfer of CO₂; between November and February (summer) there was a similar decrease with altitude, suggesting upward transfer (see figure). These patterns can be correlated with changes in the surface temperature of the sea, which decreased from May to September and increased between November and February, at an average rate of 1° C a month in each case.

Colder water has a greater capacity to dissolve CO₂ than warmer water, so

ELEMENTARY PARTICLES

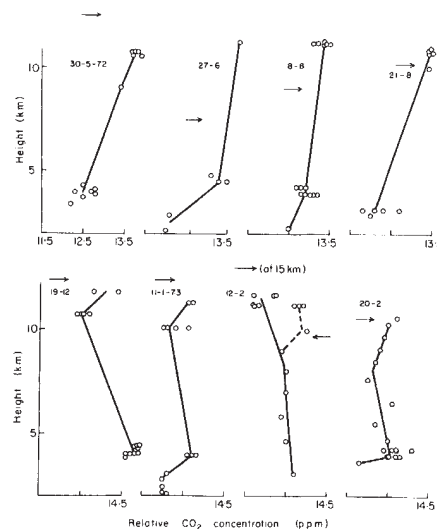
Hiatus Ahead

RECENT measurements at CERN confirm that the probability of interaction of a proton with another proton increases with energy up to at least 1,500 GeV. This could have serious implications for the Pomeranchuk theorem, one of the central tenets of high energy physics, which predicts that the probability (or cross-section) of proton-proton interactions will be almost the same as, or slightly less than, the cross-section for interactions between protons and antiprotons.

But the evidence so far indicates that the cross-section of the proton-antiproton interaction declines as the energy increases, certainly up to 50 GeV, the present limit of measurement. Unless this cross-section begins to increase with energy quite suddenly at above 50 GeV the inescapable conclusion is that the Pomeranchuk theorem is seriously contravened at energies of a few hundred GeV—in other words, the Pomeranchuk theorem is threatened because an extrapolation of existing data strongly suggests that the proton-proton cross-section is much higher than the proton-antiproton cross-section at high energies.

This concern about the validity of the Pomeranchuk theorem arises from two

these results fit into the pattern which might have been expected, namely that gas is dissolved and then given up. This presumably applies throughout the Southern Hemisphere, at least at middle latitudes, thus explaining the seasonal variations of the CO₂ gradient from the ocean surface to the tropopause.



Typical measurements of CO₂ concentration at different altitudes south-east of Australia. The dates of the measurements are included; the level of the tropopause is marked by an arrow in each case.

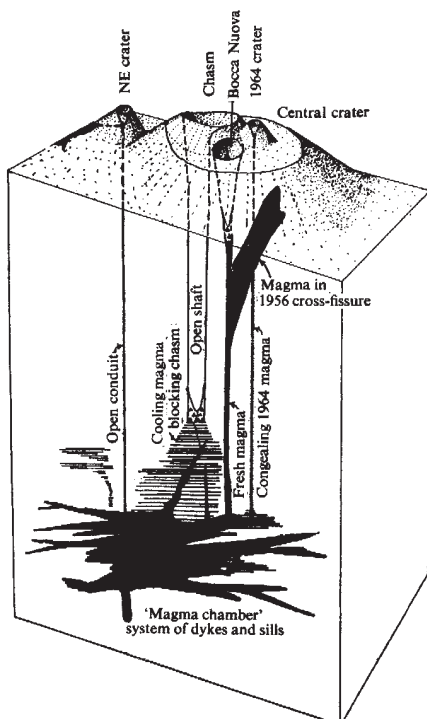
experiments carried out at the Intersecting Storage Rings by groups from Italy and the United States and members of the CERN staff; these are to be published shortly in *Physics Letters*. What these experiments have shown is that the proton-proton cross-section increases between equivalent incident proton energies of about 70 GeV and about 1,500 GeV. A year or so ago it was confidently expected that the cross-section would remain constant in this energy range. But it now seems that the total cross-section goes up from 38 mb ($38 \times 10^{-27} \text{ cm}^2$) at 70 GeV, to 39 mb at 300 GeV, 40.5 mb at 500 GeV and 43.2 mb at 1,500 GeV.

The total cross-section for protons interacting with antiprotons, on the other hand, is known to be more than 50 mb at 10 GeV but only 43 mb at 50 GeV. If this downward drift continues—and a definitive statement about this will have to await the results of experiments at the large accelerator at Batavia, Illinois, and at the 200/400 GeV synchrotron now being built at CERN or the availability of stored antiproton beams at the CERN Intersecting Storage Rings—the Pomeranchuk theory will have to be re-examined.

VOLCANOES

Model of Etna

THE eruption of Mount Etna in 1971 is the subject of a recent issue of the *Philosophical Transactions of the Royal Society*. The diagram shows the prob-



Hypothetical block diagram of the summit cone during 1971. Vertical distances are not necessarily to scale. (From Guest, *Phil. Trans. Roy. Soc. Lond.*, A274, 75; 1973.)

able distribution of magma within the summit cone in 1971 and provides an explanation of how the eruption came about.

The Bocca Nuova opened up in 1968 as a small gas vent and Guest (*Phil. Trans. Roy. Soc.*, A274, 63; 1973) suggests that it was the collapse of the Bocca in 1970 which gave rise to the 1971 eruption. The magma column below the Bocca may have burst into a fracture created during the 1956 eruption, which may then have been enlarged as a prelude to the escape of lava from the far side of the summit cone at the start of the 1971 eruption.

SEMICONDUCTORS

Low Light Levels

from a Correspondent

BECAUSE the eye, an instrument weighing 20 g and parallel-connected to an adaptive computer and several servosystems, can detect 10^{-16} W in unit bandwidth (noise equivalent power, NEP), it might be claimed that there is nothing left to do. But in an introductory review at a meeting of the Electronics Group of the Institute of Physics at Imperial College, London, on February 23, Dr T. S. Moss (Royal Aircraft Establishment, Farnborough) pointed out many deficiencies in this line of reasoning. There is, for example, ten times more radiation at $1 \mu\text{m}$ on a clear moonless night than at $0.5 \mu\text{m}$, where the eye's response peaks, and a hundred times more ($10^{-8} \text{ W cm}^{-2} \text{ sr}^{-1} \mu\text{m}^{-1}$) at $1.6 \mu\text{m}$; also the eye has a slow response and gives little range and velocity information. Furthermore, when external sources of illumination cannot be used, imaging can be accomplished in the 8 to $14 \mu\text{m}$ atmospheric window by making use of the heat of objects at ordinary temperatures, generally using cooled detectors. The need to know range and velocity has led to the development of yttrium-aluminium-garnet (YAG) lasers at $1.06 \mu\text{m}$, carbon dioxide lasers at $10.6 \mu\text{m}$ and fast sensitive detectors.

Unequalled detectivity at $10.6 \mu\text{m}$ (the figure of merit $D^* = 10^{11}$ in a 60° field of view, twice the limit imposed by illumination from an unrestricted room temperature background) was claimed for liquid nitrogen-cooled photovoltaic diodes made from the alloy lead-telluride, by Dr T. J. Waterfield (The Plessey Co. Ltd). Noise sources in these detectors and their associated amplifiers were discussed, and it was revealed that integrated arrays of detectors give uniform spectral response when used with current amplifiers. Almost as high a detectivity ($D^* = 7.3 \times 10^{10}$ in a 30° field of view) has been found in photodiodes in cadmium-mercury-telluride at $10.6 \mu\text{m}$ and this work was described by Dr J. Marine

(French Atomic Energy Commission, Grenoble) in a joint paper with Dr Motte (Société Anonyme de Télécommunications). The diodes are produced by the implantation of 250 keV aluminium ions and their subsequent annealing to give donors: the abrupt junctions have a higher quantum efficiency (50 per cent) than diffused diodes and give cut-off frequencies of 300 MHz.

Both these kinds of diode had been used by Drs C. T. Elliott and D. J. Wilson (Royal Radar Establishment, Malvern) to detect CO_2 laser radiation in a heterodyne (or strictly homodyne) system. The use of a local oscillator removes the noise from the incoherent background radiation and with a lead-tin-telluride detector a record $5 \times 10^{-20} \text{ W}$, only 2.5 times the theoretical limit set by photon noise, was needed to give unity signal-to-noise in unit bandwidth (NEP); the photovoltaic cadmium-mercury-telluride detector is only a factor of two worse but holds greatest promise for the largest bandwidths. Both photovoltaic and photoconductive cadmium-mercury-telluride detectors (from Mullard) show serious non-linearities but the latter have yielded an NEP of 10^{-18} W at 193 K, thus avoiding nitrogen cooling.

Advances in photocathodes using single crystal semiconductors were described in papers from Services Electronics Research Laboratory (SERL) and Mullard. Dr G. A. Allen described how the devices depend on obtaining a p-type semiconductor in which the bottom of the conduction band lies above the vacuum level, that is with negative electron affinity. This is achieved by coating the semiconductor surface with caesium and oxygen, which provides a thin positive space charge through which minority carriers tunnel into vacuum. In a transmission photocathode light must penetrate through a transparent medium, which forms the wall of the vacuum chamber, into an active region, usually p-type gallium arsenide a few microns thick. In order to avoid loss of minority carriers at dislocations the transparent substrate must be a crystal of matching lattice parameter. The growth of both substrate and layer by liquid phase epitaxy was described by Dr M. C. Rowlands. Their much higher quantum efficiency will probably make such photocathodes standard devices for the near infrared.

Three of the contributors dealt with detectors based on silicon technology; Dr J. M. Shannon (Mullard) described arrays based on photosensitivity junction field-effect transistors, Dr J. Dickson (Plessey) discussed limitations of arrays in general, and Dr R. Webber (SERL) discussed the theory of photodiodes operating under avalanche conditions.

Science for the Public Service

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This article is based on a speech delivered by Sir Alan to the Parliamentary and Scientific Committee at its annual luncheon on February 8, 1973.

WE have just benefited from a great national debate on the organization of applied research and development. There have been government papers, both green and white, reports by the Select Committee on Science and Technology, Parliamentary debates, and innumerable comments and articles. It has been a frank and lively discussion and I am told that we have been envied abroad by people who wish it were possible to hold a similar public airing of scientific policies in their own countries. Now that the debate is over, I believe everyone will benefit if we can achieve what the government has consequently set out to do, which is to bring closer together the people whose job it is to know the practical problems to be solved and those whose job it is to know what research is necessary to solve them and how to do it.

Balance

A good debate, yes ; but I think it has involved some risk of getting things out of balance. We must always remember, for example, that research and development are only a part of the total scientific and technical activity of the country and occupy only a fraction of the scientific manpower. Most of the qualified scientists and engineers in industry are not doing research and development at all ; they are managing, designing and looking after technical marketing and sales, which are all necessary to realize the material benefits of science and technology. Even in government, research and development are, together, only one of various scientific services. There are many others: for example, weather forecasting, operational analysis, setting and maintaining standards, surveying and mapping, monitoring of public health and pollution, testing of foodstuffs, chemical analysis, forensic science, and diagnosis of causes of accidents. Only occasionally does this kind of work make the headlines, as, for example, in the investigations of the Ronan Point and Comet aircraft disasters, or the recent identification of swine vesicular disease, but it is nevertheless work that is greatly satisfying in both its intellectual content and its contribution to public services.

Quality

A second possible risk in the debate—that of raising doubt about the quality of the research and development work—fortunately did not emerge at all. For it to have done so would have been quite without justification. The government's concern in fact was entirely with questions of organization and in particular with how to bring those who do the research and development closer to those who need its results. The high quality of the work itself was never in doubt, and we have only to look at a few recent successes to see how good it is.

The blind landing equipment pioneered by the Royal Aircraft Establishment and taken up by our industry is proving to be a world winner and is now going into many types of civil aircraft which have been accepted for automatic landings. Again, the gas centrifuge process which we have developed for enriching uranium, this time with our German and Dutch partners, is also looking most promising for commercial operation. The Advanced Passenger Train, the carriages of which have a most clever suspension system that enables them to run smoothly at great speed on ordinary track, is ahead of anything else of its kind in the world ; and because the most common transport facility that most countries have is a network of ordinary railway lines, this train should attract worldwide interest.



Sir Alan Cottrell, Chief Scientific Adviser to the government.

Similarly in many other fields. For example, in the use of very large computers for weather forecasting ; or the development of carbon fibres and other new constructional materials ; or equipment for the dispersal of oil at sea, which has been exported all over the world. We have also developed a narrow-beam long-range asdic for exploring the ocean floor ; and in the transport field, the high-temperature sodium-sulphur battery for electric propulsion is promising. There have been some striking advances made in the rearing of fish and shell-fish and this country is well advanced in the experiment of producing protein from oil by micro-organisms, as also with synthetic "steaks" from high-protein beans. We have developed some highly successful new varieties of winter wheat, which are now being exported as well as widely used in this country. The application of

clinical epidemiology to the evaluation of health care is another important advance. In yet another field, the first systematic national air pollution monitoring system in the world, developed by the Warren Spring Laboratory, has provided a model which has been copied by several other countries.

Improvement

These are just a few of our recent scientific and technical successes. There are many more. For a country of our economic size it is, I think, a very creditable performance. Can we do even better? Yes, I think we can, by achieving even closer understanding between the researcher and the user, and this is one of the reasons for our reorganization. One pitfall which we must avoid, of course, is the temptation to spread our efforts too thinly. To pioneer a new technology is expensive and we must be on our guard not to embark on glamorous and costly developments simply because they are fashionable. We also have to exert self-control in limiting ourselves only to those projects which are within our means. The newly-formed Requirements Boards will have an important part to play here, so far as governmental projects are concerned. Because we cannot afford to compete in every field the answer will sometimes lie in joint ventures with other countries; and our entry into the European Economic Community should, of course, do much to stimulate these.

One of the chief purposes in all this is the economic welfare of the country, but there is, I believe, a basic misconception when people try to relate our research and development efforts to the economic condition of the country. One of the clichés of the time is the remark that Britain is one of the heaviest spenders on research but has one of the most slowly growing economies. This is supposed to be a great mystery and paradox. But the answer is simple. Research and development can achieve great technical miracles but cannot by themselves achieve great economic miracles; for these you must also have the massive capital investment in the new industrial plant and equipment needed to embody the fruits of research and development in actual production. Research and development may be spark-plugs of the economy, but only when they are attached to an engine can you get power.

The achievement of economic growth thus lies beyond research and development itself and is a matter of investment in productive plant, which may be used for new industrial ventures made possible by that research and development. Investment, of course, raises other problems; and we are making a great effort at present to break through the stranglehold of low capital investment and low growth without running into excessive inflation and balance of payments problems. The government's recent decisions to give massive financial support to the coal and steel industries show some of the determination here. We all look forward to the success of these efforts.

Widening Horizons

If we now expand our view in space and time, extending it to Europe and to the world as a whole, the problem of generating sufficient investment capital seems likely to grow more general and more severe in the years to come. In developing countries it is already the chief factor that limits their ability to raise their standard of living by creating more jobs, and installing much needed power stations, desalination plants, and other vital equipment. In the industrialized countries it will become critical in the final years of this century for meeting the future requirements for energy and raw materials. Western Europe and Japan are already the world's biggest importers of these and their needs are con-

tinuing to rise in the face of a world situation where raw materials are being increasingly consumed, where the bargaining strength of producer countries is growing, where the United States itself is beginning to become a major importer of the same materials, and where protection of the natural environment is an increasingly important constraint. In the past, technological advance in the mining and minerals industries has been able to keep prices of basic resources fairly steady, in the face of the changing demand-supply situation, but it will be pretty hard put to keep up this balance in the future.

In a strictly scientific sense there is no possibility of running out of material resources. The point is that it will cost more and more to continue to meet our needs. At much higher extraction costs, vast reserves of oil in the tar sands and shales could be tapped. Aluminium could be extracted from clay, uranium from granite and water could be desalinated. Given the money, all these and many other things could be done, including improvements to the environment. We could provide our houses, for example, with district heating from waste sources. Or fit them out with heat pumps; or solar heaters; or fuel cells running on hydrogen made at nuclear power stations hundreds or thousands of miles away. We could carry our electricity along underground superconducting cables. We could use other means of propulsion for motor vehicles. But the overall limiting factor will be availability of capital. To take just one item as an example, a recent forecast is that this country will need 100,000 MW of nuclear electricity by the year 2000. That will mean spending nearly £1,000 million a year on nuclear plant alone once that programme is fully under way in the 1980s and thereafter.

New Technologies

These are very broad issues, of course, and will involve high policy in the European Economic Community. But research and development will have important parts to play. Improved mining techniques, the intensification of scrap recovery and recycling, the development of substitute materials, will all help to ameliorate the resources problem but each will require considerable scientific and technical effort. Whichever way these new technologies develop, it seems almost certain that they will cost more in energy to run, so that eventually the most critical thing of all will be the supply of energy. The extent to which national policies in such common fields as energy and the environment are to be coordinated in West European effort must obviously be a matter for detailed discussion within the EEC. At present we are, of course, in a good position in Britain with our coal reserves, North Sea discoveries, and nuclear developments; but looking further into the future and also to our position as a member of the European Economic Community, this is no time for complacency. The government in fact has already stressed this in its decisions to develop strong capabilities for the future in the coal and nuclear fields. It will, of course, be essential to back these decisions with strong scientific efforts, and at the same time to press forward generally with the exploration of ways of providing and using energy more efficiently and economically.

Not only are there many opportunities here for science and technology to make great contributions to the country's future well-being and to that of the European Economic Community as a whole, but it will in fact be essential to harness science and technology to do so. This is a great challenge to the whole scientific community, but my experience of government laboratories, of the research councils, industry and the universities leads me to the view that our scientists and engineers will respond to it just as willingly, as enthusiastically and as effectively as they did in the most memorable past.

Controversy about MIRVs

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The debate about how successfully the Soviet Union could eliminate United States land-based missiles with its own SS-9 launchers seems to turn on how many 1 Mt warheads the Soviet device could carry.

IN the course of the public debate in the United States on the decision to deploy anti-ballistic missile defences, a sharp, and in some cases acrimonious, disagreement emerged between those expert witnesses who contended on the one hand that the Soviet Union would by some near future date find little difficulty in employing its offensive missile forces to destroy up to 95% of the American land-based offensive missile force, and on the other those who saw great difficulties which would prevent the Soviet Union from ever becoming capable of such a thing. The latter, and probably more numerous, school found its most articulate and consistent spokesman in Professor G. W. Rathjens, whereas the former found its champion in Professor Albert Wohlstetter.

Disagreement

The gulf of genuine disagreement between the two schools appeared so great, in spite of the considerable measure of common understanding that had been reached by their principal spokesmen, at least, on the presumed values of the physical characteristics of the Soviet missile force—its size, accuracy and overall reliability were not, ultimately, in dispute—that the Operations Research Society of America (ORSA), at Wohlstetter's request, conducted an enquiry into the affair. The results of the enquiry were published in September 1971 (ref. 1) and came down very much on the side of the Wohlstetter school. But the publication of the enquiry's findings, and, indeed, the manner of the setting up of the enquiry, have led to a new controversy, part of which is reflected in a selection of letters published by the ORSA in January 1972 (ref. 2).

One remarkable feature of the original debate, as it was recorded in oral and written testimony before the United States Senate³ and elsewhere, and of the ORSA enquiry, was a steady refusal to state and analyse the problem in general terms, partly, no doubt, in the former instance because of reluctance to alienate a non-mathematical audience and partly, presumably, because of worries that information of a "classified" nature (available, it seems, to both Wohlstetter and Rathjens, if not to all their supporters) could be inferred from an analysis conducted this way. (All subsequent references to the Wohlstetter and Rathjens positions relate to testimony and papers published in ref. 3 unless otherwise stated.)

This practice, however understandable in some ways, has had several undesirable consequences for a debate of such wide public and political concern. Contentious calculations have been conducted without the basic assumptions being exposed to view; the use of secret data to arrive at public conclusions has made it all but impossible for an intelligent outside observer to decide where the truth lies when conclusions differ; and the relative importance of the physical parameters involved, insofar as uncertainties in knowledge of their values affect the general

precision and trustworthiness of the conclusions reached, has been left obscure. In an attempt to redress some of these failings, what follows is a general analysis of the problem, conducted in simple terms and, partly from necessity but also because of the openness it allows, based only upon publicly available data. The intention is to expose the structure of argument upon which all conclusions about this problem must ultimately rest. It is not intended, however, to present numerical conclusions of superior accuracy to those of either school; rather I shall show how reliable conclusions may be reached, given a knowledge of the key parameters, and demonstrate that some uncertainties are likely to persist no matter how trustworthy this knowledge is.

Probabilities

To begin with, it is necessary to establish the probability, p_k , that one perfectly reliable missile warhead (one of many presumed identical missile warheads whose accuracy is known on a statistical basis) aimed at a concrete emplacement (taken to be a point target) will so damage the emplacement as to render inoperative the missile launcher contained within it. Assuming that the theoretical distribution of miss distances of an infinite number of shots at the emplacement would be normal, and recalling that military usage ascribes accuracies to projectiles on the basis of their "circular error probable" (CEP)—the radial distance from the target within which half of an infinite number of shots fall—it follows that

$$p_k = 1 - \left(\frac{1}{2}\right)^{L^2/C^2} \quad (1)$$

where C is the CEP and L the distance from the target within which the warhead must explode in order effectively to destroy the target. L is related to the explosive energy yield of the warhead, Y , and the critical blast pressure under which the concrete emplacement will start to collapse, T , by

$$L^3 \propto Y/T \quad (2)$$

Missile warheads are, however, known not to be perfectly reliable; rather they will in general only have a definite probability (determined by test programmes of one kind or another) of reaching the target area once the decision is taken to launch them. If the probability of a missile warhead reaching the target is p_r , then the overall probability that the target will be destroyed becomes $p_r p_k$. If the missile warheads are arranged to be aimed n to a target, the overall probability of the target being destroyed becomes $1 - (1 - p_r p_k)^n$. Or, put slightly differently, the expected fraction of targets surviving an attack of n missile warheads a target is

$$(1 - p_r p_k)^n \quad (3)$$

Alternatively, if those controlling the attack have perfect information as to which of their missile warheads will fail to reach their target and are able to stagger firings to permit the

substitution of additional launchers (and reprogramming of guidance systems), the expected fraction of targets surviving an attack by the same number of missile warheads as before becomes less in general, namely $(1 - p_k)^{n p_r}$. The difference between this expression and expression (3) is rather small for large n and small p_k , but becomes significant for small n and large p_k .

In practice, those responsible for the attack cannot be expected to possess perfect information about which of their missile warheads will malfunction—at any rate not soon enough to take full advantage of the knowledge. If p_r^0 is the probability that a missile warhead will prove reliable during that phase of its trajectory about which those in control of the attack have no useful information, the expression for the expected fraction of targets surviving the attack becomes

$$(1 - p_r^0 p_k)^{n p_r / p_r^0} \quad (4)$$

(And in the event, more likely than not, that the exponential term in this expression is not a whole number, the best strategy for the attacker is to allocate the nearest lower whole number of missile warheads to all targets and one extra missile warhead each to as many targets as possible.)

Numerical values can now be attached to the various parameters involved in the solution of the practical problem, around which the controversy revolved, of the survivability of the 1,000-strong US land-based Minuteman force in the face of a hypothetical Soviet attack force of 500 SS-9 missile launchers, each capable of carrying several warheads (in multiple independently targetable re-entry vehicles, MIRVs). This statement of the problem is common ground to Wohlstetter and Rathjens; I shall ignore for the moment the fact that the Soviet Union has recently agreed to restrict its SS-9 forces to below 500.

The value of p_k depends, as outlined above, on three factors: the warhead CEP, the yield of the warhead and the "hardness" or blast resistance of the concrete Minuteman emplacement. Both schools agree about the probable value of the CEP of the SS-9, but neither publicly specify it. A rounded figure well inside theoretical limits⁴ and near the published guesses would be 460 m (0.25 nautical mile): and one can expect military planners to be confident of the CEP of their warheads to within plus or minus 30 m or so⁴. Rathjens performed his calculations exclusively on the basis of a warhead yield of 1 Mt (TNT equivalent) and Wohlstetter worked on the basis of two possible yields, 1 Mt and 5 Mt, while judging the latter to be the more relevant. The blast resistance of the Minuteman emplacement I shall take to be 300 pound inch⁻², which has been cited as a plausible figure for some years now⁵ and may well be below what can be achieved—however, p_k does not depend as strongly on blast resistance as on CEP. Wohlstetter and Rathjens do not publicly specify blast resistances, except that Rathjens's estimate, according to Wohlstetter, was two-thirds higher than his own. Obtaining p_k from standard graphs⁶ of accuracy/yield relationships, it turns out that

$$p_k(5 \text{ Mt}) = 0.98 \pm 0.01 \text{ and } p_k(1 \text{ Mt}) = 0.77 \pm 0.04$$

where the error estimates arise from probable uncertainties in CEPs.

Reliability

According to Wohlstetter the same overall SS-9 reliability probability, p_r , was assumed by himself and Rathjens: he does not specify its value. The ORSA conducted a calculation on the basis of $p_r = 0.73$ —a seemingly somewhat arbitrary figure. The firmest and most authoritative public estimate of p_r made

before the debate was that of Fink⁷. He put $p_r = 0.66$ for the United States's own pre-MIRV warhead missile forces. For the SS-9 force I shall use the rounded figure of 0.7, to take some account of possible improvements since Fink's article was published. (Fink's figure was for the United States retaliatory force and it might be said that for a force used to initiate a conflict the overall figure should be higher—given that surprise is on the side of the attacker. This may be true, but it should be noted that even small temporary delays in "countdown" could seriously interfere with the side contemplating surprise attack using staggered firings, whereas these would scarcely matter for a retaliatory strike.)

This overall probability can be seen as a product of four contributing factors⁷: (i) the probability that the missile launchers are available; (ii) the probability that they are ready; (iii) the probability that they complete boost phase reliably; and (iv) the probability that the payload is reliable in the post-boost phase.

If all these four probabilities are taken to be equal, they will each have a value of 0.91. In practice, however, it would be surprising if the first and second of these probabilities were not somewhat larger than this average figure, given that an attacker can choose to make his attack when circumstances most favour him. And it would be equally surprising if the last of the probabilities were not somewhat smaller than the average, given the relatively complex engineering system associated with a multiple warhead payload; in any case, the value of the post-boost phase reliability probability cannot be as well known by those planning an attack as the values of the first three, even when multiple warhead payloads are commonplace, because the Partial Test Ban Treaty prohibits full (complete with warhead detonation) flight testing of missile warheads.

Rather than treat the four probabilities as equal, as does Fink (the ORSA report put the first two equal and high, 0.95, and the last two equal but slightly lower, 0.90), it would seem a slightly better approximation to the actual situation to assume that planners might tend to assign equal values to the first two (0.95), and 0.91 to the third—all fairly firmly—and a smaller and less precise estimate (0.86 ± 0.05) to the fourth. (Fink⁷ suggests that a conservative military planner might attach a 10% downwards uncertainty to each of the four reliabilities: placing a 6% error bracket on one of them only does not by comparison seem to be ascribing an unusual degree of caution.)

Throw-weight Capability

It now remains to establish how many separately targetable 1 Mt warheads a MIRV SS-9 can carry. Official US spokesmen claim that the SS-9 can launch either one 25 Mt warhead or three 5 Mt MIRV warheads³: these figures, and, presumably, the booster capacity implied, were never disputed in the debate. Rathjens, however, suggests that the SS-9 could, alternatively, carry four 1 Mt MIRV warheads, but Wohlstetter (and the ORSA) insist that this is too few, without actually making explicit what they regard as the true figure. An independent estimate can be made with the help of some simple calculations. If, as is usual*, the weight of a warhead is assumed to depend upon the yield raised to some power, the above official data suggest a two-thirds power law, which when applied to the 1 Mt case indicates that an SS-9 launcher is capable of a payload of nine 1 Mt MIRV warheads. But this sort of calculation ignores the weight of the post-boost phase missile warhead release system which is part of all MIRV payloads. Applying the two-thirds power law to the known case of the conversion of the United States Minuteman II launcher from a payload of 2 Mt to three MIRV missile warheads of 0.2 Mt each, the weight of the warhead release system

* See, but for a different power law, Kent⁸. According to the ORSA¹, Rathjens also employed a power law approach to this question.

for three 0.2 Mt warheads comes out as equivalent to that of two extra 0.2 Mt warheads. If the weight of the Soviet SS-9 warhead release system per warhead in excess of unity can be taken to be the same, then that for six 1 Mt warheads would weigh roughly as much as two extra 1 Mt warheads (but the mechanism for three 5 Mt warheads only as much as 0.25 extra 5 Mt warheads). Seven then seems a more plausible figure than that of Rathjens, or that resulting from a crude application of the two-thirds power law, for the number of 1 Mt MIRV warheads an SS-9 could carry.

It is now possible to tackle, but in an open manner, the various calculations performed by Wohlstetter, Rathjens and the ORSA. Taking the number of US missile emplacements to be 1,000 and the size of the Soviet SS-9 attack force to be 500 launchers (or its equivalent: one of the recent strategic arms limitation agreements puts an upper limit of approximately 300 on the SS-9 force until 1977 at least) and assuming first of all no Soviet ability to substitute for failures, expression (3) predicts that the percentage of US missile launcher emplacements likely to survive might be taken by Soviet planners to be $7 \pm 2\%$ for a 1 Mt SS-9 MIRV force ($p_k = 0.77 \pm 0.04$, $p_r = 0.7 \pm 0.04$, $n = 7$). For a 5 Mt SS-9 MIRV force ($p_k = 0.98 \pm 0.01$, $p_r = 0.7 \pm 0.04$, $n = 3$), the surviving fraction becomes $20 \pm 2\%$. Wohlstetter gives 7.3 and 16%, respectively; Rathjens gives 25% for his 1 Mt SS-9 (four warhead) MIRV attack, the ORSA does not investigate the non-substitution possibility.

On the other hand the assumption could be made, possibly more realistically, that the Soviet Union will be able to substitute for failures up to and including, but not beyond, the booster separation stage, which is reached about 5 min after launch and after the launcher has travelled approximately 1,000 km of its flight for a missile launcher of the SS-9 type* (indeed all indicators suggest that the United States is itself currently unable to substitute for failures of this type). Under these circumstances expression (4) yields $5 \pm 2\%$ for the fraction of US missile emplacements which the Soviet Union may expect to survive its attack with a 1 Mt SS-9 MIRV force ($p_k = 0.77 \pm 0.04$, $p_r = 0.7 \pm 0.04$, $p_r^0 = 0.86 \pm 0.05$). For a 5 Mt SS-9 MIRV force ($p_k = 0.98 \pm 0.01$, $p_r = 0.7 \pm 0.04$, $p_r^0 = 0.86 \pm 0.05$) the survival rate becomes $12 \pm 4\%$. Wohlstetter gives a bald 5% for both cases; the ORSA gives 5% for the 5 Mt SS-9 MIRV case on the basis (like Wohlstetter, presumably) not only of a higher overall reliability than that assumed here but also of an assumed Soviet ability to substitute for some (50%) of its post-boost phase failures. Rathjens does not consider the possibility of failure substitution. It should be noted that the fraction of missile emplacements surviving an attack is not the same as the useful surviving percentage of missile launchers; this is less by a factor of 0.70, in our case, to allow for unreliability.

Implications

Several points of general interest emerge from the above. First, uncertainties as to CEP values are relatively unimportant for high p_k values, that is for large yield missile warheads; a 6% uncertainty in CEP contributes only a 1% uncertainty to p_k for a 5 Mt warhead but a 5% uncertainty to p_k for a 1 Mt warhead. It follows that Wohlstetter's insistence in the debate on the importance of missile warhead accuracies and the likelihood of their improvement; thus, he alleged, placing Minuteman emplacements even more at risk was just a little beside the point, particularly in the context of a discussion centring on 5 Mt warheads which Wohlstetter contended was the most realistic case: a p_k of 100%, together with the values for the SS-9 reliability factors quoted above, would still result in 11% of Minuteman emplacements surviving a 3×5 Mt attack. The non-substitutionable failure rate, not the accuracy, is the important limiting factor here. For the theoretically possible 7×1 Mt case, uncertainties in non-substitutionable failure rate and in CEP contribute about equally to uncertainties in the final result.

This hitherto relatively unsung importance of reliability, it might be noted, places the significance of the Partial Test Ban Treaty as an arms limitation agreement in a much more favourable light than it has recently enjoyed.

Second, it follows that placing a single figure without error brackets on the survivability rate, as seems to have been the habit of all parties concerned in the debate, is an almost meaningless activity. (Wohlstetter seems to have been the worst offender here; at one point³ he quotes a survivability rate to an implied accuracy of 1 to 2%.) It seems a fair pre-supposition that military planners responsible for analysing the consequences of attacks of this sort will not be in possession of precise figures for the reliability and accuracy of their forces, and that a prudent planner contemplating attack will be more influenced by the upper limit of his estimates of emplacement survivability than the lower. It is typical of calculations of this sort, of course, that a planner contemplating defence might consider it prudent to concentrate on the lower limit of survivability—an example of the so-called "worst case analysis" ascribing to an adversary, without any apparent justification, capabilities that the adversary himself could not prudently rely on.

Third, it turns out that the above numerical conclusions, although not based upon much more than rough estimates of some parameters, broadly speaking agree more closely with those of Wohlstetter than with those of Rathjens, at least insofar as they indicate that the fitting of multiple independently targetable missile warheads to land-based missile launchers will reduce to a considerable degree the relative value of these launchers as a means of retaliation to an attack designed to take advantage of their vulnerability. A pre-MIRV SS-9 force of 500 launchers would have left at least 500 Minuteman emplacements intact, whereas, according to my calculations, the same force fitted with 3×5 Mt MIRV warheads, and launched using the substitution technique for system failure as far as, and including, the boost phase, would leave at most only 160 emplacements intact, containing perhaps 110 reliable launchers—a total of 330 missile warheads. Greater reliabilities for large warhead MIRVs and their launchers, or a greater subdivision of payloads into more numerous, accurate, and reliable smaller missile warheads, would have the effect of making the trend more marked. This prospect of a gradually diminishing utility of fixed emplacement land-based missile launchers as a retaliatory force, particularly in the absence of anti-missile missile defence of emplacements, is bound further to encourage the development of new forces of a mobile and therefore less pre-targetable sort and to result in more weight being placed on the development of existing mobile types such as those based on submarines and aircraft.

And finally, although the differences between Wohlstetter and Rathjens can be regarded as being primarily differences in judgment about how Soviet weapons policy may develop, and it is too soon to say definitely who is right, it should also be noted that Rathjens's results would nonetheless not differ significantly from Wohlstetter's if his method of calculating the number of 1 Mt MIRV warheads an SS-9 launcher could carry were the same as that used here.

¹ *Operations Research*, **19**, No. 5 (1971).

² *Operations Research*, **20**, No. 1 (1972).

³ *Hearings before the Committee on Armed Services* (United States Senate, April–June 1969).

⁴ *Impact of New Technologies on the Arms Race* (edit. by Feld, B. T., Greenwood, T., Rathjens, G. W., and Weinberg, S.), 81 (The MIT Press, Cambridge, 1971).

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Late Miocene Desiccation of the Mediterranean

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This article presents evidence that the Mediterranean Sea was a desiccated deep basin some 6 million years ago.

THE presence of an evaporite deposit of Late Miocene Messinian age under the Mediterranean Sea was discovered two years ago by the Deep Sea Drilling Project (DSDP) Cruise Leg XIII (refs. 1–4). To start with, our postulates were greeted with disbelief, but detailed analyses of samples and syntheses of regional geology during the past two years have led to a confirmation of this apparently preposterous idea. We shall present in this article our principal conclusions and some of the critical evidence. Detailed documentation will be published in due course⁵.

Three Models

The origin of the Mediterranean evaporites could be accounted for by three different models. In the first, there was evaporation of a deep water Mediterranean basin, which received constant inflow from the Atlantic so that its brine level was maintained at or slightly below the world wide sea level. The second involves evaporation of a shallow water Mediterranean basin, which, similarly, received constant inflow from the Atlantic so that its brine level was maintained at or slightly below the world wide sea level. According to the third, desiccation of a deep Mediterranean basin, isolated from the Atlantic, took place, so that evaporites were precipitated from playas or salt lakes whose brine levels were dropped down to thousands of metres below the Atlantic sea level.

The first may be called the “deep water, deep basin model”⁶ and the second the “shallow water, shallow basin model”⁵. The third, namely the desiccated deep basin model, is, however, the one we prefer.

Late Miocene Basin Geometry

Geophysical evidence—chiefly the basin-wide distribution of an acoustic reflector⁸ (Fig. 1) which has been identified by drilling as the top of the Mediterranean evaporites⁵—clearly indicates that the Late Miocene Mediterranean had a configuration not greatly different from that of today. The surface of the reflector conforms more or less to the contours of the intricate submarine topography, indicating that the Mediterranean basin had already been created when the evaporite was being deposited.

More convincing evidence is provided by stratigraphical and palaeontological studies. The strata underlying the evaporites are deep marine pelagic sediments. During the DSDP Leg XIII, Middle Miocene pelagic marls were cored from sites 126 and 129 (see Fig. 2) in the Ionian Basin^{1,5}, and lower Upper

Miocene (Tortonian) marls were sampled from site 121 in the Alboarn Basin⁵. The correlative Tortonian marl underlying the Upper Miocene evaporites (Messinian) of Sicily has been proved to be a deep water deposit by a study of its benthonic foraminiferal faunas⁹. Deep marine pelagic marls of Lower Miocene age are also known on the island of Pianosa in the Tyrrhenian Sea¹⁰. These facts clearly indicate that the Mediterranean basins were already deep before the salinity crisis.

The strata directly overlying the Messinian evaporites are also deep marine pelagic sediments. These earliest Pliocene strata contain a benthonic ostracod fauna, which could only live in ocean bottom below 1,000 m (ref. 11). The associated benthonic Foraminifera are likewise indicative of a deep marine environment of deposition⁹. The fact that of the deep-swimming planktonic genus *Spheroidinellopsis* is the dominant (up to 90%) microfauna lends further credence to the concept of a deep Mediterranean in the earliest Pliocene⁵. Additional data in support of deep marine sedimentation immediately after the salinity crisis have been provided by the oxygen isotope measurements⁵. As geophysical considerations exclude the possibility of a catastrophic subsidence⁵, the only alternative is the sudden drowning of a desiccated deep basin.

Pelagic oozes are, in fact, intercalated in the Messinian evaporites. At site 134, to the west of Sardinia, we cored a

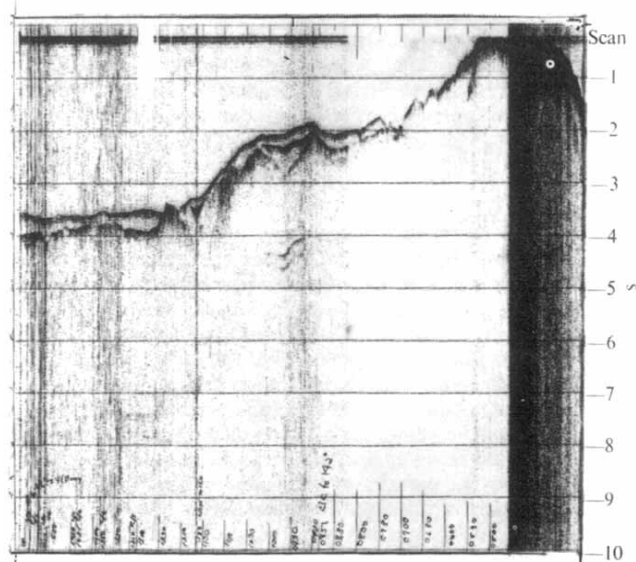


Fig. 1 The Mediterranean reflector. This strong reflector corresponds to the top of the Upper Miocene evaporite formation. The relief of the reflector conforms the bottom topography, suggesting that the evaporites were deposited in a basin similar in topography to that of the present Mediterranean.

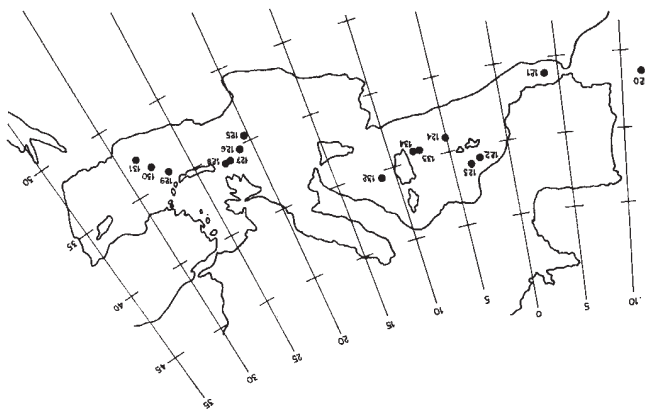


Fig. 2 Drill sites of Leg XIII of the Deep Sea Drilling Project.

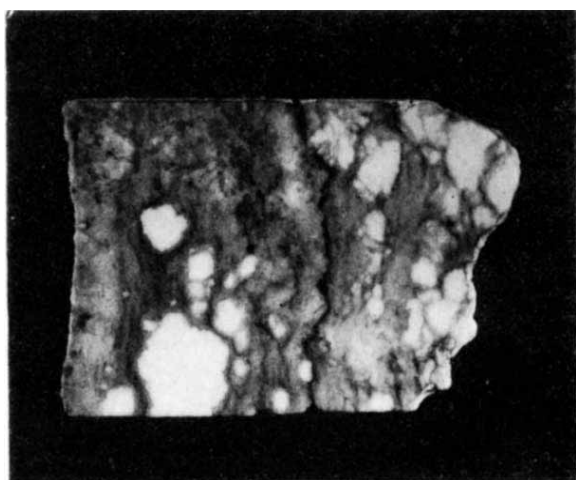


Fig. 3 Nodular and "chicken wire" anhydrite, characteristic precipitates from ground waters under hot and arid coastal flats.

deep marine foraminiferal ooze of Messinian age between two sterile halite layers⁵. Similar marl oozes have been encountered between anhydrite layers at site 124 south of Mallorca⁵ and in the Messinian evaporites now uplifted and exposed on Sicily⁷. Where a Late Miocene Mediterranean basin was located near land, as in the case of Periadriatic Trough in Italy or Khania Basin in Crete, deep water turbidities are interbedded with the evaporites^{12,13}.

If the Late Miocene Mediterranean had been shallow, the marine sediments associated with the evaporites would have been shallow water deposits. This is not the case. The occurrence of deep marine sediments below, above, and within the evaporite sequence proves conclusively that the Mediterranean was already a deep basin during Middle and Late Miocene times.

Environment of Deposition

Although the Upper Miocene marine sediments are deep water deposits, mineralogical, petrographical, sedimentological, and geochemical data strongly suggest that the Upper Miocene evaporites were, on the whole, not precipitated from a deep brine pool; they were formed chiefly in shallow waters or subaerially.

The presence of anhydrite as a dominant sulphate mineral in the Mediterranean evaporites is a strong argument against the idea of a deep water genesis. Anhydrite, CaSO_4 , is the higher temperature polymorph of calcium sulphate. Below a critical temperature of phase transition, gypsum ($\text{CaSO}_4 \cdot 2\text{H}_2\text{O}$) should be precipitated. This critical temperature is 58°C for

the $\text{CaSO}_4 \cdot \text{H}_2\text{O}$ system¹⁴, but is about 20°C for calcium sulphate precipitated from brines saturated with sodium chloride (ref. 14 and F. W. Dickson, personal communication). Deep aqueous bodies rarely acquire a temperature high enough to precipitate anhydrite. Even the Dead Sea, where the surface temperature exceeds 30°C and may locally exceed 40°C , anhydrite is not formed¹⁵; only gypsum is found on the shores and on the bottom of it.

The occurrence of anhydrite in the form of nodules proves further that it has been crystallized in a subaerial environment. Nodular anhydrite is found today exclusively on hot and arid coastal flats, called *Subkhas* in Arabic, where it is precipitated at the ground water table, 0.5 m or so beneath the surface at temperatures above 35°C (J. Schneider, personal communication); this diagenetic mineral replaces carbonates and sulphates deposited earlier and may form a whole anhydrite bed in which only wisps of organic materials remain. The resulting structure has been given a vulgar name of "chicken wire anhydrite" by geologists working in the petroleum industry and has been considered by some to be an infallible criterion of subaerial crystallization¹⁶ (see Fig. 3).

A careful examination of the cores from site 124, to the south of Mallorca, reveals that the process of formation of evaporite there consists of several cycles of inundation and desiccation (Fig. 4). Each cycle commences with the deposition of laminated carbonates. The predominant carbonate is a dolomite rich in organic matter, and its evenly laminated structure is indicative of deposition in quiet, and probably fairly deep, bodies of water. The carbonates yield either marine micro and nanofossils, or a brackish fauna and flora; they were either

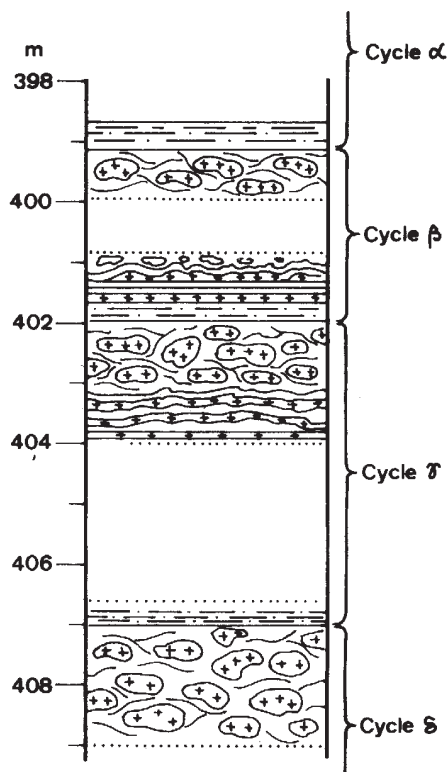


Fig. 4 Schematic diagram of desiccation cycles in hole 124. 1, Laminated carbonates; 2, interlaminated dolomite and anhydrite; 3, stromatolitic deposits; 4, nodular anhydrite.

deposited in a deep sea or in a large brackish lake. Overlying the laminated carbonates are interlaminated dolomite and anhydrite; the laminations are less well defined near the top, a trend suggestive of increasing agitation as the water became shallower. Stromatolites appear near the top of the depositional sequence; they formed only when the brine pool was sufficiently shallow to permit the growth of algae. At the very top of every cycle is the nodular anhydrite, presumably a product of subaerial diagenesis. The anhydrite is covered in turn by very evenly laminated carbonates of the next cycle of inundation.

The halite rock samples from beneath the Balearic abyssal plain also show signs of desiccation. Euhedral "hopper crystals" precipitated in brine pools have been partially replaced by anhedral clear crystals during times of subaerial exposure. A desiccation crack is present, filled with clear halite (Fig. 5). Intercalated in the halite is a cross-laminated foraminiferal silt that has apparently been deposited in an aeolian environment.

Playa deposition is further supported by investigations of stable isotopes. Fig. 6 shows the range of oxygen isotope values of the Mediterranean evaporites¹⁷. The great variability argues against the idea of precipitation from a brine pool. In fact, the wide range is very similar to that of the playa evaporites, by contrast with the narrow range characterizing the marine evaporites.

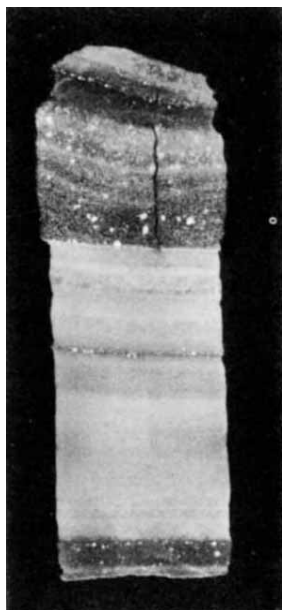


Fig. 5 Halite core from site 134.

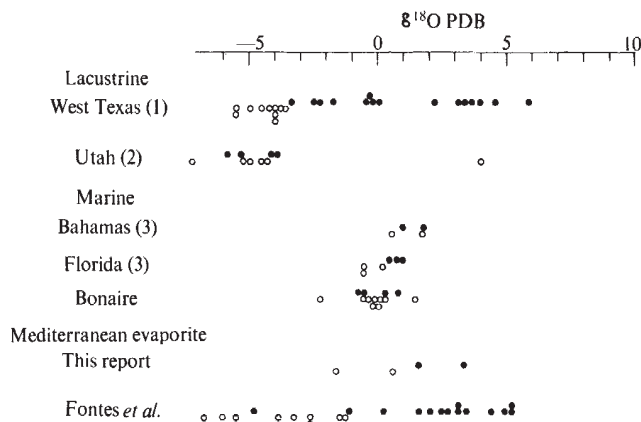
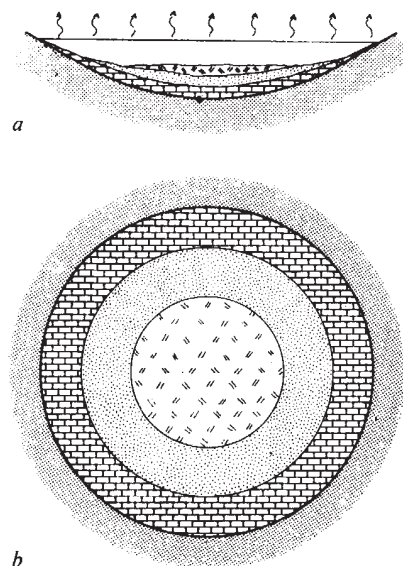
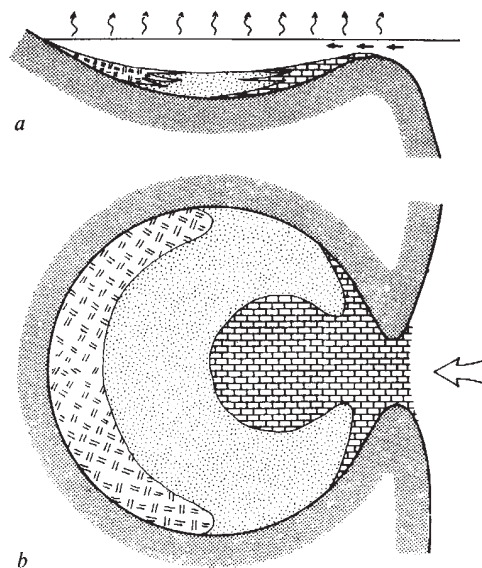


Fig. 6 Oxygen isotope range of the Mediterranean evaporites by comparison with marine and playa evaporites. ●, Dolomite; ○, calcite.



Carbonates Gypsum Halite

Fig. 7 Idealized bull's eye pattern of evaporite distribution typical of isolated basins. a, Cross-section; b, map.



Carbonates Gypsum Halite

Fig. 8 Idealized tear drop pattern of evaporite distribution typical of partially restricted basins. a, Cross-section; b, map.

Finally, the distribution pattern of the Mediterranean evaporites is not at all what has been predicted on the basis of a deep water model; deep water evaporites should have a tear drop pattern rather than the bull's eye pattern typical of playa deposits. The latter is well known: a gradual desiccation of a playa should result in concentric zones of saline minerals^{18,19}, the outermost carbonate being the first salt to precipitate from a brine, and the inner core a most soluble salt (Fig. 7). By contrast, a restricted deep marine basin with an opening at one end would have less soluble salt deposited proximal to the opening and more soluble near the distal end (Fig. 8). If the

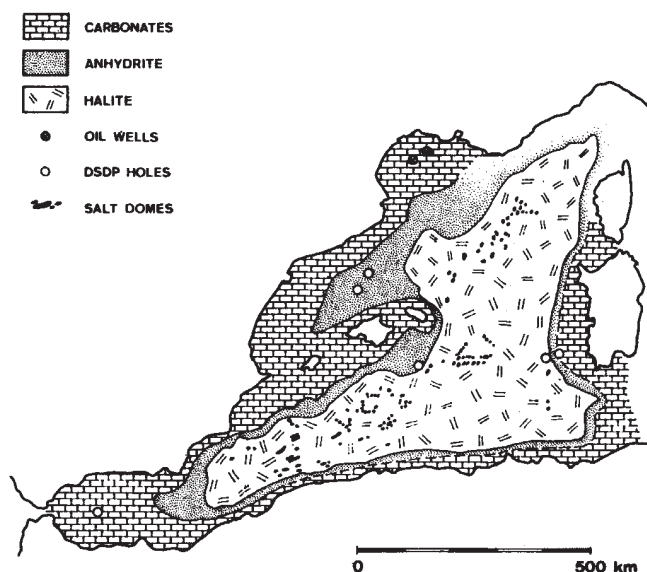


Fig. 9 Probable distribution of evaporites in the western Mediterranean Balearic Basin.

deep water model were applicable, we should have found potash salts and halite in the eastern Mediterranean, but only gypsum and/or dolomite in the western basins. The drilling results showed no evidence of such a "tear drop" pattern. In fact, drilling confirmed the suggestions arising from seismic profiling, namely that halite lies only in the deepest part of each Mediterranean basin and that the evaporite distribution conforms to a bull's eye pattern (see Fig. 9).

Geomorphological Evidence

A desiccated Mediterranean during the Late Miocene dictates that the base level of erosion must then have been thousands of metres below the sea levels. Shelf seas should have withdrawn from continental areas, and coastal plains and newly exposed shelf should have been dissected by rejuvenated streams. They should have cut canyons of steep gradients hundreds of metres into a slightly older marine sediment, and should have left alluvial and terrestrial clastics in the channels. Such a stream system should have been drowned during the final submergence of the desiccated Mediterranean in the earliest Pliocene.

Such an important regression has indeed been deciphered from the available geological records on land. In southern France, for example, a marine sequence, ranging up to Upper Miocene Tortonian, has been cut by a deep channel system. The channels were filled with alluvial gravels, which in turn underlie marine Pliocene sediments. The event recorded by the channel cutting has been known to stratigraphers as the Pontian regression, but its cause was unknown until we formulated our model of Late Miocene desiccation.

Pontian regression has also been reported in Egypt, where the River Nile at Aswan cut a gorge 200 m below sea level. It should be recalled that Aswan is a long way upstream, some 1,250 km from the coast; buried beneath the Marine Pliocene and Quaternary alluvial sediments of the Nile Delta area is a grand canyon comparable to the Grand Canyon of Colorado¹¹. Similar buried gorges have been found in Libya, Syria, Israel, and other Mediterranean lands.

We might extend our reasoning a step further. When the Mediterranean was desiccated, the river channels should not only have cut the shelf margins, but should also have continued down towards the flat bottom of the Late Miocene playas, and extended 2,000 to 3,000 m below the present sea level. Post-Miocene sedimentation probably did not fill these channels completely, so drowned river valleys of Late Miocene age should be present on modern continental margins as submarine canyons.

Such submarine canyons have indeed been found, indenting the continental margins of southern France, Italy, Corsica, Sardinia and North Africa^{8,22,23}. They were sculptured by streams and subsequently drowned during the Early Pliocene marine transgression. Similar submarine canyons are also present in the eastern Mediterranean⁸.

The extension of Miocene streams down to the continental rises left behind floodplain silts and channel gravels. Such terrestrial clastics have been cored in a hole (site 133) at the foot of the continental rise to the west of Sardinia⁵, and similar detrital deposits are present on the periphery of the Messinian evaporite basins of Sicily²⁴. These occurrences are only explicable in terms of the model of basin desiccation.

History of Evaporite Deposition

The Mediterranean Sea, excluding the Black Sea, has an area of 2.5 million km² and a water volume of 3.7 million km³. The annual loss by evaporation is 4.7×10^3 km³. The annual precipitation is 1.2×10^3 km³ and the annual volume delivered by river influx is 0.2×10^3 km³. The net loss is thus 3.3×10^3 km³ yr⁻¹. If the Strait of Gibraltar were closed today, the present Mediterranean could be evaporated dry in about 1,000 yr.

The thickness of halite salt that could be precipitated isochemically from one basinful of Mediterranean waters (averaging 1,500 m in depth) is, however, only about 20 m. Even if all the salt was deposited within a restricted basinal area covering one-third of the total, the resulting deposit should still be only 60 m thick. Yet the seismic record shows that the halite deposit under the Mediterranean may be two or three kilometres thick⁵. It is unlikely the freshwater influx from the rivers supplied all the salts, so repeated marine invasions must be postulated. Both the drilling results and the study of the correlative salt deposit of Sicily indicated that this was indeed the case; the Mediterranean was dried up and refilled repeatedly during the few million years represented by the Messinian stage. It should be recalled that the refilling of a basin could not have been instantaneous. Considering the inevitable evaporative losses during the transient stage, several basinfuls of water must have found their way across before the basin was filled up; thus each marine incursion into the Mediterranean might have brought enough salt to deposit a few hundred metres of halite in basinal areas. A detailed computation of the material balance budget led us to conclude that eight or ten marine invasions, represented by the interbeds of marine marls in the Upper Miocene evaporite-formation of Sicily, could have been sufficient to account for all the salts under the Mediterranean abyssal plains⁵.

Although the flood gate at the Strait of Gibraltar apparently swung open and shut repeatedly during the Late Miocene, the gate was irreparably crushed at the beginning of the Pliocene. The earliest Pliocene sediment of the Mediterranean is a deep marine ooze, characterized by a cold water bottom fauna. Although the recurrent Messinian "refills" might have been related to spill-overs caused by eustatic rise of the world wide sea level, the final deluge was probably related to a rifting movement along the Azores-Gibraltar fracture zone. The initial gap was deep enough to permit the entrance of deep Atlantic bottom faunas into the Mediterranean. The Strait of Gibraltar was gradually shoaled during the Pliocene, and eventually the supply of deep Atlantic waters was cut off, causing the extinction of the cold Mediterranean benthonic faunas. Yet the Strait is still open sufficiently to permit the reflux of partially evaporated Mediterranean waters so as to keep its salinity only slightly above that of the open ocean.

Explaining an Improbable Fact

We have presented in this article the evidence which led us to conclude that the Mediterranean was a desiccated deep basin some 6 million years ago. The full documentation of the data will be published in our cruise report⁵. We realize that our

deduction seems improbable because none of the desert basins today are comparable in size or depth to a desiccated Mediterranean. Yet the improbable fact that the Mediterranean Sea is underlain by a salt deposit demands an improbable explanation. We welcome comments from our colleagues in the Earth and biological sciences, particularly if some of their own observations bear out, or disprove, the idea that the Mediterranean was a desert during the Late Miocene.

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Voltage Dependent Charge Movement in Skeletal Muscle: a Possible Step in Excitation-Contraction Coupling

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It is suggested that a link in excitation-contraction coupling involves the movement of a fixed amount of charge free to move between different locations across the membrane.

STARTING with the experiments of Huxley and Taylor¹ and Huxley and Straub² the view has developed that, under physiological conditions, contraction in skeletal muscle is triggered by depolarization of the membranes of the transverse tubular system (T-system)³. This network of tubules extends throughout the cross-section of a fibre⁴ and is positioned at regular intervals along the fibre length⁵, thus providing a means whereby a change in surface potential can be rapidly transmitted into the interior^{6,7}. There is also evidence⁸⁻¹⁰ indicating that the final stage of excitation-contraction coupling involves a release of calcium ions into the myoplasm from its intracellular storage location, the sarcoplasmic reticulum (SR); the elevated Ca²⁺ then activates the contractile proteins¹¹. It has not been clear, however, how a change in potential across the tubule membrane could bring about the release of Ca²⁺ from the neighbouring SR. The experiments reported here are an attempt to detect an ionic current or movement of

charge across the T-system membrane which could play a role in triggering this response.

Voltage-clamp Measurements

In our experiments sartorius muscles from English frogs, *Rana temporaria*, were cooled to approximately 2° C in a solution designed to eliminate virtually all of the time and voltage dependent changes in sodium and potassium currents. The composition was 117.5 mM tetraethylammonium (TEA) chloride, 5 mM RbCl, 1.8 mM CaCl₂, tetrodotoxin (10⁻⁶ g ml⁻¹), and 1 mM Tris-maleate buffer to give a pH of 7.1. Movement due to contraction was practically eliminated by making the solution hypertonic with sucrose addition, 350 to 583 mmol to 1 l. Sucrose hypertonicity appears to have little effect on calcium release as the heat of activation is decreased only 10 to 20% on adding 450 mmol sucrose per 1 normal Ringer's solution¹². Voltage-clamp measurements were carried out using the three microelectrode technique described by Adrian, Chandler and Hodgkin¹³. In this method two voltage sensing microelectrodes were positioned at intervals 1 and 21 from the end of a particular fibre and a third electrode was used for passing current, as is shown in Fig. 1A. The membrane current density at x=1 is related to the difference in potential $\Delta V (= V_2 - V_1)$ by the relation

$$i_m = 2\Delta V / 3r_i l^2 \quad (1)$$

in which i_m is current and r_i is internal resistance per unit length of fibre. Feedback electronics were used to set the

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potential V_1 at a command level E_c . The voltages V_1 and ΔV were displayed on a 'Tektronix 565' oscilloscope and photographed; in addition, one of the signals, usually ΔV , was integrated successively over intervals of 1–2.5 ms with a 'Technical Measurement Corporation Computer of Average Transients', Model 1000, or sampled at 0.5 to 2.5 ms intervals using a 'Fabritek Signal Averager', Model 1072, with a 20 μ s filter in the input circuit.

Fig. 1*B* shows two sets of photographed records of V_1 (upper trace) and ΔV (lower trace). In each case a depolarizing voltage pulse of 41 mV was applied to the fibre; in the upper records (*a*) the pulse was superimposed on the holding potential, -79 mV, and will be denoted (-79 mV, 41 mV), whereas in (*b*) a conditioning step was applied 100 ms before the start of the pulse so that the 41 mV pulse was superimposed on a potential of -126 mV (-126 mV, 41 mV). Both ΔV records show an initial 2 to 3 ms surge, corresponding to the capacitive transient, followed by a relatively flat trace. Although the two sets of records look very similar, small differences are seen on closer examination. This is shown most clearly by taking the difference between the two records; Fig. 1*C*, trace *c*, shows the result of summing the difference four times. It is apparent that the 41 mV step from -79 mV produced a small, outward current over and above that seen with a step from -126 mV. The extra component decayed with time to a steady level which corresponds to a slight nonlinearity in the current-voltage curve.

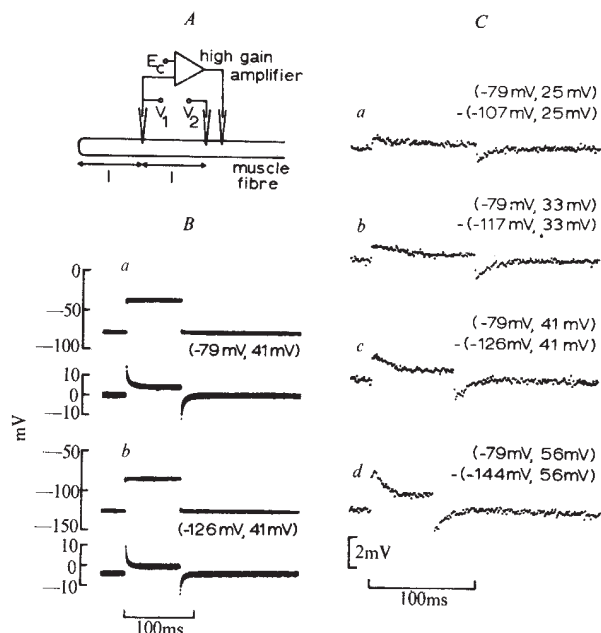


Fig. 1 *A*, Diagram of the three microelectrode technique. *B*, Single traces of V_1 and ΔV from fibre 113.2, holding potential = -79 mV, $l=172 \mu\text{m}$: *a*, pulse applied from holding potential; *b*, potential stepped to -126 mV 100 ms before start of pulse. *C*, Photographs of Fabritek display, fibre 113.2. Each trace represents the difference in ΔV between records such as *Ba* and *Bb*, summed four times to improve signal-to-noise. The vertical calibration refers to a single difference. The sampling rate was 1 kHz.

Two other features shown by the four records in Fig. 1*C* are of interest. First, the magnitude of the extra outward current increased as the voltage pulse was increased. Second, on repolarization there was a tail of inward current, and the area above the tail was approximately equal to the area under the transient part of the extra outward current, as indicated in Fig. 3*A*.

As a quantitative relationship of the type shown in Fig. 3*A*

has strong implications about the nature of the extra charge movement, the effect of pulse duration on the "on" and "off" areas was studied. One such experiment is illustrated in Fig. 2. Signal averaged records of (-80 mV, 47 mV) minus (-127 mV, 47 mV) for 10, 20, 40 and 80 ms pulses are shown in Fig. 2*A*. It is clear that the amplitude of the tail was small for short pulses and that it tended to reach a steady level as the duration was increased. In all cases, the area of the "off" response seems to match the area of the "on" (Fig. 2*B*).

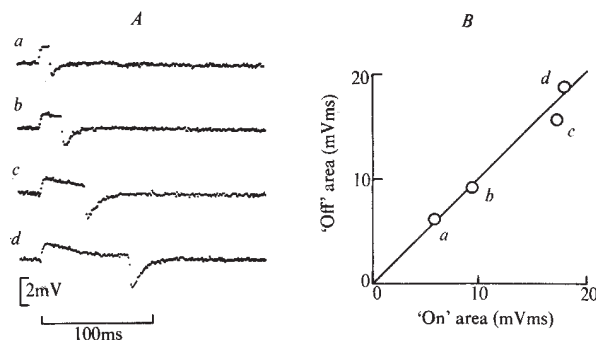


Fig. 2 Effect of pulse duration on charge movement. *A*, Photographs of Fabritek display, fibre 111.1, holding potential -80 mV, $l=205 \mu\text{m}$. Each trace shows the difference (-80 mV, 47 mV) minus (-127 mV, 47 mV) signal-averaged four times, 1 kHz sampling rate. Pulse duration: *a*, 10 ms; *b*, 20 ms; *c*, 40 ms; *d*, 80 ms. *B*, Comparison of "off" areas with "on" areas, from part *A*. The 45 degree line represents perfect agreement. The final level in trace *d* was subtracted from all records in calculating the "on" area.

That the results fit the relation (area of "off") = (area of "on") suggests that the transient extra current is due to the displacement of a quantity of charge trapped in or near the membrane. It might also be possible, however, that the agreement of areas is fortuitous and that the current is due to a change in membrane conductance, for example, to calcium. The current seen with the step depolarization would then be the sum of a constant outward current and a slowly developing inward calcium current. Although several arguments oppose this view, the most direct is based on the type of experiment in Fig. 2*A*. The "on" current 10 ms after the start of the pulse shows little or no decline and, presumably, the lack of development of the hypothetical inward current. The peak tail current following a 10 ms pulse (2*Ad*), however, is already one-third maximum as determined in 2*Ad*. This lack of correspondence as well as the agreement of areas in Figs. 2*B* and 3*A* argues against a conductance change and in favour of a charge displacement for the underlying mechanism.

Relationship between Charge and Membrane Potential

The next question is whether there is a simple relationship between the amount of charge which moves and membrane potential, V . Other experiments, not given here, show that little if any extra charge movement occurs for changes in potential in the range V less than about -100 mV. Defining $A(V)$ as the area of the extra transient (which is proportional to charge) we may set $A(V)=0$ for $V < -100$ mV. The relatively small value of $A(-79 \text{ mV})$ can then be estimated by comparing areas for pulses from -98 mV and -79 mV, represented by the squares in Fig. 3*A*; this value for $A(-79 \text{ mV})$ is shown in Fig. 3*B* and was used to adjust the other points in the figure. The theoretical curve was drawn according to the expression

$$A(V)/A_{\max} = [1 + \exp(-(V - V')/k)]^{-1} \quad (2)$$

with $V' = -53 \text{ mV}$, $k = 8 \text{ mV}$ and $A_{\max} = 22 \text{ mV ms}$. Equation (2) is the same functional form used by Hodgkin and Huxley

to describe steady-state inactivation of the sodium conductance in squid axons¹⁴. Results from this and four additional experiments gave an average value of -49 mV for V' , 11 mV for k . Experiments in isotonic solutions below the contraction threshold gave values for k similar to those observed here, but depolarization was insufficient to determine V' .

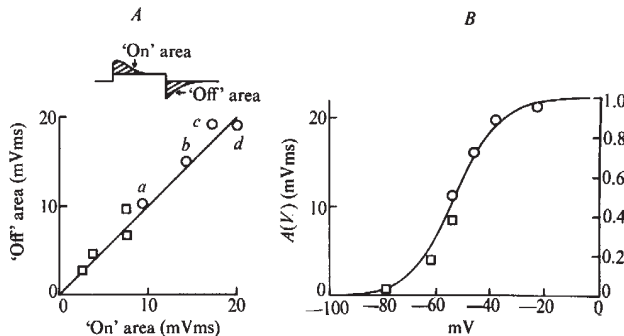


Fig. 3 Effect of membrane voltage on charge movement. *A*, Comparison of "off" areas with "on" areas. ○, Measured from the experiment in Fig. 1C; □, measured from a similar set of traces taken somewhat earlier from the same fibre. The purpose of this run was to compare depolarizations to -62 and -54 mV from two conditioning levels -79 and -98 mV. The 45 degree line represents "on" area = "off" area. *B*, $A(V)$ versus V . The symbols ○ and □ represent the average areas from the same symbols in part *A*, adjusted for the small but finite value of $A(V)$ around the holding potential. The scale on the right is normalized to range from 0 to 1 and serves as the ordinate for the calculated curve.

Our results are clearly consistent with a model in which there is a fixed amount of charge free to move between different locations across the membrane or between locations which see different proportions of the total membrane potential. (A mathematically indistinguishable model can be based on rotation of permanent dipoles.) In the simplest case one might imagine just two positions, 1 and 2, which see a difference in voltage αV proportional to the total membrane potential. When the voltage is very negative, all the charged groups would be in position 1; with depolarization some of the groups would move to position 2; on repolarization the groups that moved to 2 would return to 1. These movements of charge would produce transient currents resembling those observed in the present experiments.

The model can be made quantitative in the following way. If f_1 is the probability of the group being in position 1, f_2 in position 2, the Boltzmann expression gives

$$\frac{f_1}{f_2} \propto \exp[-\alpha z F V / RT] \\ = \exp[-\alpha z F (V - V') / RT] \quad (3)$$

where z is the valence of the particle, V' is the potential at which $f_1 = f_2$, and F , R , and T have their usual meaning. As $f_1 + f_2 = 1$, equation (3) can be solved for f_2 to give the expression in equation (2) where $k = RT / \alpha z F$. The value of 8 mV for k in Fig. 3B corresponds to $|\alpha z| = 3.0$; the average value of 11 mV gives $|\alpha z| = 2.2$. Thus, if α were close to unity, the charged group would have a valence of magnitude two to three.

Properties of the Charge Movement

Although the physiological role of this charge movement is unknown, it is interesting to note that it has certain properties which could make it useful as a trigger mechanism. First, the range of membrane potentials in which the charge movement occurs is the range in which many of the electrical and mechanical properties of muscle are strongly regulated. Second, the currents produced by the charge movements are sufficiently small that they would not interfere with the normal propagation of an action potential. Third, the movement of charged groups from a resting position to a new location on depolar-

ization, and the return on repolarization, could involve a physical motion of some tens of Angström units, a motion which could correspond to turning a molecular trigger on and off.

The charge movements described here may in many ways be qualitatively similar to "gating" currents in squid axons, recently found by Armstrong and Bezanilla¹⁵, which may play a role in activating sodium channels. Quantitative differences, however, are clear; in muscle the amount of charge per cm^2 of total membrane (surface + T-system) is roughly three times that in squid axon and the time course is twenty to one hundred times slower. This makes it improbable that the charge movements in muscle would be linked to gating sodium channels.

A more likely speculation which can serve as a guide in planning future experiments would be to suppose that the charged groups were located in the membranes of the T-system and that their displacement constituted a step in excitation-contraction coupling. If the groups were attached to long molecules which extended to the adjacent projections of the SR terminal cisterna membrane¹⁶, they would provide a means by which the potential across the wall of the T-system could be sensed by the SR; in fact, movement of the molecules could directly regulate the release of Ca^{2+} . In the simplest form one might imagine that the rate of Ca^{2+} release could be proportional to $A(V)$ (see Fig. 3B) or some function of $A(V)$. The threshold for contraction, -50 to -55 mV in normal Ringer⁷ and possibly more negative in the solutions used here (see refs. 17, 18), would be the potential at which the rate of release was just greater than the rate of uptake. Along these lines it is of interest to estimate the number of charged groups per cm^2 of muscle membrane. In the experiment in Fig. 3 the electrical capacitance was measured according to the method of Adrian *et al.*¹³. Using a value of $1 \mu\text{F}$ per cm^2 of membrane^{19,20}, the values of A_{max} and k in Fig. 3B give 300 charged groups per μ^2 of membrane (surface + T-system). This number is roughly similar to the number of electron dense "feet" joining the T-tubule and the periodic projections of the SR membrane; these occur with a frequency of 700 per μ^2 of tubule membrane²¹. Thus, it would be numerically possible for approximately one charged group to be associated with each "foot".

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African Infant Precocity and Some Social Influences during the First Year

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In African infants precocious in both mental and motor test performance during the first year of life, environmental factors contributed at least 25% to test score variance. Even in the first year, therefore, social factors must be considered in evaluation of psychological development of these infants.

SEVERAL studies¹⁻³ have led to the widely-held belief that sub-Saharan African infants are precocious in their psychological development. These studies leave much to be desired methodologically⁴. Nevertheless, the preponderance of evidence suggests that compared with infants of the United States, of England, and of France, African infants during their first year of life are precocious in motor (neuromuscular) development, and possibly in mental (perceptual-sensory) development as well.

This precocity seems to be less marked for infants from westernized and/or middle class family backgrounds than for those from traditional and/or lower class families⁵⁻⁷. These observations imply that the precocity in sub-Saharan African infants, which might be thought to be genetically based⁷, may also be substantially influenced by social factors.

The purpose of this study, therefore, is (1) to investigate whether or not there is development precocity, using more reliable individual measures based on longitudinal study of the infants, and (2) to assess the relationship between selected social and demographic variables and psychological development.

The sample comprised sixty-five infants born between July 1 and December 31, 1969, residing in a periurban Kikuyu, predominantly agricultural community of 4,500 people, approximately 25 miles from Nairobi. An estimated total of 100 infants was born during this period and ninety of their mothers agreed to participate in the study. Seven infants were lost to the study because their families moved from the community or for some other reason their data are incomplete. Eighteen infants served as controls for another segment of this study and were not tested longitudinally. Of the sixty-five sample infants, thirty-four were male and thirty-one female. Twelve were first-born; among the others, the median number of older siblings was

five, including eleven subjects in the sample having eight or more older siblings.

The Bayley Test⁸ in standard form was used for all infant testing. The items used in this test divide into two scales, mental (perceptual-sensory function) and motor (neuromuscular function). Testing was done by a British-trained Kenyan nurse who was bilingual (Kikuyu, English) and whose family lived near the village. Ten non-study infants were used in the training procedure. Interobserver item agreement on the test for the final two pretest infants reached 85%.

Each infant was tested at approximately 2-month intervals, beginning in January 1970, on at least four and at most eight occasions. Testing was done in the morning or in the afternoon at times when the infant was judged alert and cooperative. All infants were breast-fed during the entire period, the older infants receiving supplementary food. Evidence of gross malnutrition was absent on two clinical examinations provided at the beginning and end of the study.

Demographic and social information was obtained from interviews of the mothers conducted by two college-level Kikuyu-speaking young women. In addition to routine demographic information, data were collected on family structure, educational achievement, economic status and density of the household. A check list was used to obtain a brief description of the material amenities available within the household.

Within this rapidly changing community, there was noticeable variation in the social and economic circumstances of the families for such variables as the amount of land available to each family, the educational level of the parents, the maintenance of traditional practices (for example, the practice of polygamy) and the contact with the urban environment of Nairobi.

Test results were first analysed by comparing the performance of Kikuyu infants on individual test items against United States standards⁸. For each item, the number of infants able to perform the task described by the test item earlier than the United States median age for performance of that task, and the number unable to perform the task at the United States median age, were tabulated. Kikuyu infants surpassed United States performance on thirty-eight items for the mental test and twenty items on the motor test; they lagged behind United States performance for seven items in the mental test and two items on the motor test. There was no apparent pattern in the type of items which differentiated the two groups, though our impression

is that Kikuyu infants lagged behind the United States performance on items involving implements which were less familiar in their environment than in the United States.

These findings suggest that if Kikuyu infants were scored by United States standards using items passed at a given age, they would tend to show performance superior to United States expectations. Each infant was given a developmental quotient (DQ) for the mental and motor tests using United States conversion tables⁸ at each of his first four test sessions. The four DQs were averaged to get a single score for each infant on the mental and motor tests. The mean mental score for sixty-five Kikuyu infants was 108.4 (s.d.=24.4); the mean motor score was 129.5 (s.d.=38.9). The expected United States average score would be 100.0, with expected standard deviation ranging from 16 (if test-retest reliability is 100%) to 8 (if test-retest reliability is 0%). The variabilities of the Kenya sample are somewhat but not significantly larger than that expected in a United States sample. On both the mental and motor tests, Kikuyu youngsters score significantly better (*t* test, $P < 0.01$) than would be expected in a United States group.

The precocity on the motor test corroborates the findings of Geber and is quite similar to the Uganda findings of Kilbride⁹ obtained using the Bayley Test in a cross-sectional sample. The finding of precocity on the mental test, though not unanticipated, was surprisingly clear, and consistent with reports for African infants and United States black infants^{10,11}. The Kikuyu infants performed better than the United States black group on the mental test, as well as on the motor test. This finding may be attributable to the fact that in this study testing was done by a bilingual, bicultural tester, from the same ethnic group as the infant. This may be important in infant testing, especially for infants between the ages of 6 and 12 months, because fear of the stranger is likely in this period of development. It should also be noted, however, that maternal caretaking within the Kikuyu community involves more physical contact with the infant in the first 6 months of life, and therefore might also account for those differences (additional data on maternal behaviour will be presented elsewhere).

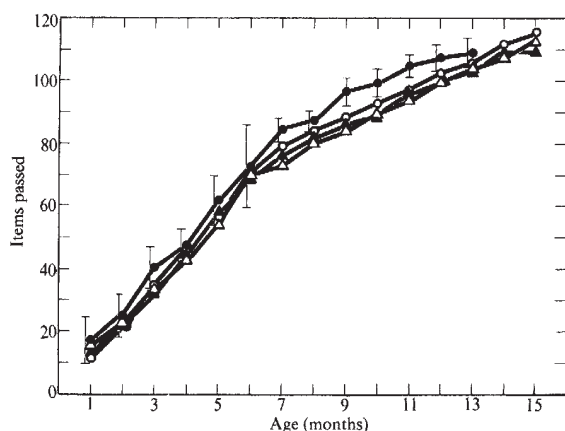


Fig. 1 Comparison of mental test performance (Bayley) for Kikuyu (●), UK white (○), US black (▲), and US white (△) infants during the first year.

Another way to present these data is shown in Figs. 1 and 2. The grand total of 376 test sessions (sixty-five infants, four to eight sessions per infant) was subdivided by the age of the infant to the nearest month at the time of testing. Mean scores were calculated for each month. Fig. 1 presents the mean number of items passed at each age for the mental test, and in Fig. 2 for the motor test. The vertical line represents a positive and negative deviation of one standard

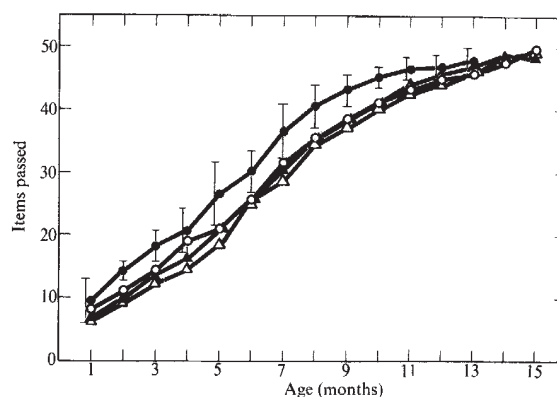


Fig. 2 Comparison of motor test performance (Bayley) for Kikuyu (●), UK white (○), US black (▲), and US white (△) infants during the first year.

deviation. It is obvious that the Kikuyu mental and motor scores exceed combined United States white and black¹¹ and United Kingdom white¹² scores at all points during the first 12 months of life.

We used a polynomial regression and a repeated measures design¹³ to examine the relationship between test performance and selected social and demographic variables. A Kenya-Kikuyu curve was computed for both mental and motor scores and a test score for each infant was calculated relative to that curve. For convenience, these scores were standardized to a mean of 100 and a standard deviation of 16.

These scores were then used as the dependent variables in a step-wise multiple regression analysis¹⁴ using as independent variables thirty-three selected social and demographic variables including such items as sex, birth order, age, education of parents, an evaluation of the modern attitudes of the mother, degree of contact with the urban environment of Nairobi, family structure, modern amenities available within the household, economic status of the family, and the household density within different age groups.

Table 1 Multiple Regression Analysis of Bayley Test Scores with Demographic Factors

(N=65)			
Mental		Motor	
Multiple correlation coefficient = +0.51		Multiple correlation coefficient = +0.45	
	Correlation factor vs test		Correlation factor vs test
Economic	+0.37 *	Number of individuals aged > 40	+0.30 *
Modern amenities	+0.36 *	Economic	+0.26
Number of household members aged 21-40	-0.22	Number of household members < 3 yr	-0.23
Number of household members aged 13-20	+0.20	Sex of infant ♀ > ♂	+0.17

* $P < 0.05$.

The results are shown in Table 1. The four indicated variables contributed the largest share to multiple correlation coefficient (+0.51) for the mental test score. Two of these variables, economic status and modern amenities available, yield individual correlation coefficients of 0.37 and 0.36 with mental test score, each significant at the 5% level. Infants in families with greater economic resources and with more modern amenities such as calendars, clocks, books, and so on, performed better on the mental test.

The same type of analysis for motor test score indicates that four of the variables made the largest contribution to the multiple correlation coefficient of +0.45. The only significant positive correlation, between the number of indi-

viduals in the household past age 40 and motor ability, suggests that additional caretaking of the infant by an older woman or grandmother contributed to infant motor development.

Further corroboration of the relationship between the test scores and social and demographic variables is shown in Tables 2 and 3. Those infants born to fathers of higher income, more training, and education score higher on the mental test than do infants born of fathers with less income, training, and education. Similarly, for the motor scores, those infants with fathers in the upper economic range score higher than other infants. In the case of the density of the household, those infants where there are two or more individuals aged 41 to 60 score the highest, whereas those without such individuals score the lowest.

Table 2 Relationship of Mental Test Scores (Kenya Norms) to Selected Social Factors

Father's education		Kenya DQ
	N	
Standard 7-8, Form 1-11	30	105.6
No information	10	100.5
No education	10	97.8
Standard 1-6	15	89.9
Cash income		Kenya DQ
	N	
Yes	34	102.1
No	24	89.3
No information	7	104.4
Father's occupation		Kenya DQ
	N	
Farmer, > 2 acres	6	106.0
Position requiring training	29	104.6
Position requiring no training	11	92.5
Farmer, < 2 acres	8	85.3
No information	11	107.4

The body of results for both mental and motor tests does not support the observations of Geber⁶ suggesting that infant precocity is negatively related to social class, and to the observations of Bayley¹¹ which indicate that there is no relationship between performance and social class. While it is difficult to reconcile the results of this study with the findings of these other studies, the differences found between this study and other studies may be due to sampling procedure and testing methodology. Our sample consisted of approximately 90% of the available infant population and was drawn from the general community and not from a hospital or clinic population. Our testing was repeated several times on the same infant during the infant's first year, in contrast to cross-sectional methods employed in these other studies.

Table 3 Relationship of Motor Test Scores (Kenya Norms) to Selected Social Factors

Presence of individuals > 41		Kenya DQ
	N	
Two	15	106.9
One	17	103.7
None	28	95.2
No information	5	93.1
Cash income		Kenya DQ
	N	
Yes	34	102.7
No	24	89.5
No information	12	100.4

A definitive explanation of why sub-Saharan African infants have precocious mental and motor development during their first year of life, and why their test performance is apparently influenced by the economic level of their families and other social factors, cannot yet be given. From

what is known about traditional Kikuyu marriage customs¹⁴⁻¹⁶, it is possible that the bride-wealth system has produced assortive mating on the basis of social and economic status. This assortive mating over many generations could lead to the developmental differences observed, even in the relatively restricted range of social and economic differences observed currently within this community. Regardless of the selection pressures that may have been operative in the past, however, we have found that contemporary social and economic conditions are positively related to the infant's psychological development in the first year.

This is particularly interesting if the observations of Parkin and Warren¹⁷ are correct. They found very few significant differences between African infants of high income families, African infants of low income families, and European infants of middle class background born in Kampala, when examined in the first five days of life, using a modified Prechtl¹⁸ examination procedure. None of the differences reported favoured one group over the other. Assuming that Kikuyu infants are similarly non-precocious at birth, then our findings of precocity developing during the first year, while conceivably genetically determined, lend support to theories emphasizing environmental influences on infant development during the first year of life.

Another possible environmental influence is nutrition, but there was no evidence for gross malnutrition among the infants, and no infants were lost to the study because of illness or death. Whether malnutrition of mothers, related to the economic level of the family, caused intrauterine deprivation of some infants, is not known. But in any case malnutrition would be expected to diminish the performance of the Kikuyu babies.

In agreement with others¹⁹, we recognize that genetic factors account for a large proportion of the variability of mental and motor test performance especially in the infant's first year. The remaining variance (approximately 25%), however, is not random, but can be associated with identifiable social and demographic variables predictive of precocious psychological development.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Radio Emission from the Close Binary b Persei

We report the detection of faint intermittent radio emission from the close binary star b Persei. Concurrent observations at 2,695 and 8,085 MHz were made with the NRAO interferometer on spacings of 900, 1,800 and 2,700 m. The total observing time was 14.4 h, mostly in short periods between February 7 and February 15, 1972. Further brief observations were obtained on April 28 and June 21, 1972.

Radiation from the direction of b Persei was seen during about 5 h. The position of the intermittent radio source coincides closely with the position given in the SAO catalogue for b Persei:

$$\alpha_{1950} = 04 \text{ h } 14 \text{ min } 28.5 \text{ s} \pm 0.13 \text{ s}, \delta_{1950} = +50^\circ 10' 28'' \pm 2''$$

(Radio)

$$\alpha_{1950} = 04 \text{ h } 14 \text{ min } 28.443 \text{ s}, \delta_{1950} = +50^\circ 10' 28.94''$$

(Optical)

The very close positional agreement and the intermittent nature of the radio emission are convincing evidence that b Persei is a radio star.

The source was seen at 8,085 MHz for a few hours on February 7–8 and February 11–12, at an average flux density of 0.012 ± 0.004 f.u. There may have been radiation at 2,695 MHz during the event of February 7–8, with a flux density of 0.005 ± 0.003 f.u., but the detection is by no means certain. At all other times, the flux density was less than 0.008 f.u. at both frequencies. The temporal structure of the radio emission is well shown by the observations made during the event of February 11–12. No radiation was seen from 1914 UT until 2214 UT (February 11). From 2214 UT until 0246 UT (February 12), radiation at an average level of 0.012 f.u. was seen at 8,085 MHz. No radiation was seen during the 2 h following 0246 UT. During this entire time (about 10 h), the b Persei field was observed intermittently at intervals of 25 min out of each hour.

Clearly, the source flared to an observable intensity for about 5 h on at least two occasions. The data are still sparse, but they suggest that the b Persei radio emission is not unlike that of Algol (β Persei)^{1–4}, although it is much fainter. The essential points of similarity are the flare-like variability of the radiation, and its greater strength toward shorter wavelengths.

The close binary b Persei joins β Persei^{1–4}, β Lyrae^{1,2} and α Scorpii B^{5,6} in the class of binary systems with flaring radio emission, but without known X-ray emission. It is an ellipsoidal variable with a period of 1.52738 day; its light curve has an amplitude of 0.06 mag at visual wavelengths⁷. Heard⁸ has confirmed earlier work indicating that the orbital elements of the system are probably changing with time. As in the case of β Persei, it is tempting to suspect that the radio emission is related in some way to binary mass exchange phenomena.

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The Missing Planet

THE idea that the asteroids are fragments of an exploded planet was first put forward by Olbers more than 150 yr ago and has since been widely accepted. Recently Ovenden¹, in an attempt to account for Bode's Law, has argued persuasively that a planet of 90 $M_\oplus$ vanished between Mars and Jupiter some 16 m.y. ago. According to this point of view the asteroids are remnants of this event.

Other possibilities have been suggested, however, for the origin of the asteroids. For example, Kuiper² suggested that they are remnants of successive collisions which occurred between a few primaeval planetoids; Alfvén³ and others regard the asteroids as being in the process of forming a single planet by accretion. The difficulty of disrupting a planet of 90 $M_\oplus$ was mentioned by Ovenden. Here we eliminate mechanisms which are incapable of providing the energy needed both to break up the hypothetical planet and to remove most of its mass beyond the Solar System. The mechanisms for disrupting a planet might be chemical, gravitational or nuclear in nature.

In the chemical case, to dissipate a planet of mass M and radius R , an energy $E \sim GM^2/R$ is required, where G represents the gravitational constant. For a mean density comparable to that of the Earth, $R \sim 4.5 R_\oplus$ and $E \sim 8 \times 10^{48}$ erg, or $\sim 1.5 \times 10^{13}$ erg g⁻¹. This seems to exclude the possibility of a chemical mechanism: the detonation energy of TNT, for example, is $\sim 5 \times 10^{10}$ erg g⁻¹.

Gravitational tidal forces due to the Sun or Jupiter are negligible at a heliocentric distance of 2.5 a.u. The missing planet might conceivably have strayed within the Roche limit of Jupiter. Such a close passage would greatly disturb, if not disrupt, the Galilean satellite system, which, according to Ovenden's theory, required 2×10^9 yr to settle into the observed resonances. Consequently the missing planet could not have strayed so close a mere 1.6×10^7 yr ago.

Nuclear effects might be the heat released by radioactive decay somehow converted to kinetic energy, fusion, or a chain reaction. Radioactive decay is a slow process and if an explosion is to result the energy generated must be stored. Thus a high pressure core must develop, constrained by a solid mantle until bursting occurs. The crushing strength of rock is $\sim 10^9$ erg g^{-1} , so even if the entire planetary mass were involved in containing the central force, breaking point would be reached long before the required 1.5×10^{18} erg g^{-1} was available. Consequently any explosion would lack the energy required by a factor $\sim 10^4$.

A similar objection applies to the hydrogen fusion reactions with the exception of $^2\text{D} + ^1\text{H} \rightarrow ^3\text{He} + 5.5$ MeV which has a reaction time of only ~ 6 s. But this reaction is only significant at temperatures above 5.4×10^8 K and this is unattainable in planetary interiors. More generally, masses less than $2.4 \times 10^4 M_\oplus$ are too small to become hot enough for nuclear reactions.

Assuming a relative ^{238}U abundance throughout the planet equal to the terrestrial crustal abundance, there is just enough energy to disperse the planet if the ^{238}U could be assembled into a super-critical mass. In addition a chain reaction has the advantage that the necessary force could be developed in less than the travel time of shock waves through the planet. But it is clearly improbable that, in nature, enormous numbers of sub-critical masses of ^{238}U could be assembled and brought together simultaneously within the planet.

There remains the possibility, mentioned by Ovenden, that the $90 M_\oplus$ might have been in the form of a ring. For the ring to dissipate, the particles must either leave the Solar System or be absorbed into the Sun.

In the first situation the energy difficulties are as severe as in the planetary case (1.9×10^{42} erg required) with the additional problem that a suitable mechanism for dispersal is even more difficult to conceive. In the second case the ring particles are required to lose virtually all their angular momentum over a very short time scale (say, $< 10^6$ yr). The only effective mechanism for systematic angular momentum loss is the Poynting-Robertson⁴ effect. To be removed in 10^6 yr or less, the particles must have been < 0.02 cm in diameter; but for the ring to have persisted for the previous 4.5×10^9 yr, they must have been ≥ 600 cm in diameter. No mechanism seems to exist whereby bodies of this size could exist for so long and yet suddenly disintegrate on a time scale $< 10^6$ yr.

We conclude that the destruction of a planet or ring in the recent past, with the mass required by Ovenden's theory, is physically improbable: one must look elsewhere for an explanation of Bode's Law and the existence of the asteroids.

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Martian Centre of Mass — Centre of Figure Offset

A DIFFERENTIATED crust and possible internal convection are suggested by this first report of a martian centre of mass-centre of figure offset. Martian topography from Earth based radar observations¹⁻³ has been reported for latitudes extending from about 22° N to about 16° S. These observations reveal a gross topography which is relatively insensitive to latitude and which shows variations in excess of 10 km (Fig. 1). Local topographic variations inferred from the Mariner ultraviolet spectrometer observations⁴ are consistent with the radar data.

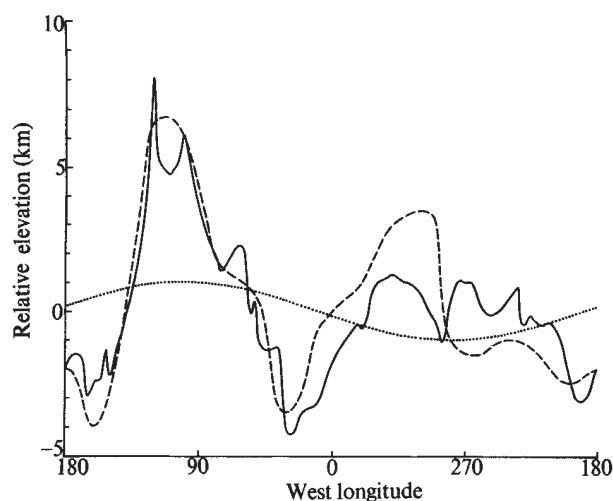


Fig. 1 Relative elevations of the martian surface at $\approx 15^\circ$ S (—) and $\approx 11^\circ$ N (---) as a function of west longitude^{2,3}. The first harmonic of the more accurate topography at 15° S (...) is also shown. The amplitude and phase of this first harmonic determine the magnitude and orientation, respectively, of the centre of mass-centre of figure offset.

Because these topographic data, deduced with respect to the centre of gravity, are complete in longitude along latitude bands close to the equator, some estimate can be made of the magnitude and direction of the centre of mass-centre of figure offset of Mars. The most accurate topography is that reported³ at about 15° S latitude. This has a surface resolution of 1.3° in latitude and 0.8° in longitude with a precision in surface height measurement approaching 75 m in regions of high radar reflectivity.

A Fourier analysis of these data reveals a first harmonic, corresponding to an offset of about 1 km, with the centre of figure displaced, with respect to the centre of mass, in the general direction of Tharsis (Fig. 1). The Tharsis region, a broad highland standing about 5 km above the mean equatorial relief, is the site of major volcanic features on Mars, including Nix Olympica and the three aligned volcanic features known as North, Middle and South Spots^{5,6}. Near the location diametrically opposite to Tharsis is the Hellas basin, the largest circular basin on Mars (about 2,000 km diameter) with a depth^{7,8} of at least 3 km.

The martian centre of mass-centre of figure offset strongly suggests⁹ that the planet has a differentiated crust, lighter than the subcrustal material and thicker under the Tharsis highlands than under the region between Hellas and Syrtis Major. For a difference of less than 10% between the density of the crustal rock and the mean density of Mars, the difference in crustal thickness must be greater than 20 km.

Mars is not unique in possessing a centre of mass-centre of figure offset. Offsets of comparable magnitude are known for the Earth^{10,11} and the Moon^{9,12}. These offsets are clearly associated with the asymmetry of the terrestrial continent-ocean basin and lunar highland-mare distributions. There is a wealth of geologic, seismic and gravimetric evidence¹³ that the terrestrial continental crust is lighter and thicker than the oceanic crust. As well as the lunar offset, there is additional evidence that the far side crust beneath the lunar highlands is lighter and thicker than the near side crust. This can be seen from the differences in chemical composition¹⁴ between the highland and mare material and the absence of a significant positive gravity anomaly¹⁵ in the front side highlands with respect to the maria, from which we may infer the thickness of crust beneath the highlands even on the far side.

Asymmetric crustal distributions are most likely produced by large scale internal convection during or after the period of crustal differentiation¹⁶. Convective motions of subcrustal material would lead to an accumulation of lighter crustal

material above regions of downwelling, making the crust thicker there and thinner in the region of upwelling. A less likely mechanism for producing asymmetric crustal distributions is the action of a few large impacts. Massive throwout of crustal material from an impact crater and subsequent partial filling of the crater with heavier subcrustal material could produce a centre of mass-centre of figure offset.

In the case of the Earth it is now generally accepted that some form of mantle convection is responsible for the present distribution of the continents. If, in the case of the Moon, the asymmetry were due to impacts, the thinnest crust should be found in the region of the largest impact features. This, however, does not seem to be the case. The largest front side impacts, Imbrium and Orientale, lie roughly 60° and 130°, respectively, from the location of the thinnest crust at about 35° east longitude, as implied by the observed⁹ offset. The recent discovery¹² of an unfilled, Imbrium-sized basin on the lunar farside further implies that early large impacts on the Moon occurred with no less frequency on the far side, making it highly unlikely that they produced the observed asymmetry in lunar crustal distribution. Thus past internal convection seems⁹ to be responsible for the lunar offset.

The occurrence of the large martian volcanoes in the Tharsis region strongly favours an internal convective origin for the asymmetric crustal distribution and resultant offset. Thus we expect that the Tharsis region is the site of downwelling convective motions with the ensuing accumulation of a relatively thick, light crust. The associated volcanism could then result from frictional heating along local fracture zones stressed by the downward motion. The morphology⁶ and partial alignment of the martian volcanoes in the Tharsis region is qualitatively similar to that of the composite, or strato, volcanoes associated with downwelling regions on Earth, although the analogy is crude at best. We note that the morphology of the martian volcanoes is inconsistent with the massive flooding that would be expected in a region of upwelling. The Mariner 9 imagery^{5,6} also revealed other evidence of extensive volcanic and tectonic modifications of the martian surface which might reflect deep internal processes. The location of the Hellas basin near the direction of the thinnest crust, as implied by the offset, might suggest that an impact produced the asymmetric crustal distribution. But such an impact alone could not account for the volcanism at Tharsis, although it could have influenced a pattern of internal convection. That the latter might be the case may be indicated by the nearly diametrically opposite positions of the Tharsis volcanic region and the Hellas basin.

The magnitude of the low order components of the martian gravity field¹⁷ and their correlation with topographic variations and consequently with volcanic regions may also be evidence for internal convection. But correlations of gravity and topography could equally well be interpreted as the support of a topography by rigid material at depth. Finally, quasi-circular topographic features observed by Mariner 9 in the polar regions have been interpreted (B. C. Murray and M. C. Malin, unpublished) as evidence for polar wandering, which might also be expected if internal convection were active on Mars.

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Plutonic or Metamorphic Equilibration in Apollo 16 Lunar Pyroxenes

A FELDSPATHIC microbreccia from the Descartes region of the lunar highlands contains an unusual assemblage of pyroxene fragments. The numerous angular grains exhibit striking subsolidus exsolution textures that indicate a very advanced state of equilibration between the participating phases. Previous work on the subsolidus behaviour of lunar pyroxenes has been confined chiefly to submicroscopic exsolution. This required resolution by X-ray diffraction analysis which could not provide the specific major and minor element compositions of each phase.

The implications of finding textures of this type depend upon two alternative hypotheses. Some strongly zoned basaltic lava pyroxene crystals may have been fragmented and then re-equilibration of the fragments occurred at relatively high metamorphic temperatures during microbreccia formation, say in ash flow. Alternatively, a thick, fractionated plutonic complex has been disrupted from within the primitive lunar highlands crust, and the pyroxene exsolution developed within that pluton has been preserved within the microbreccia formation. We prefer the latter hypothesis.

Two polished thin sections (67075,43 and 67075,48) have been examined. The rock sample (219 g) was collected during the Apollo 16 mission, from the southeast rim of North Ray crater. It is a feldspathic microbreccia in which both clasts and matrix are very rich in fragmented, calcic

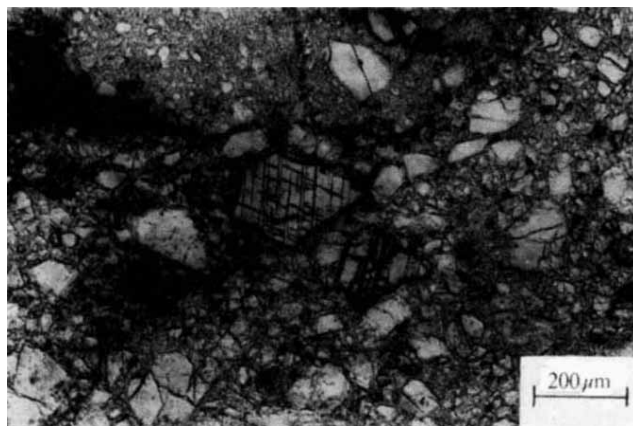


Fig. 1 Photomicrograph of thin section of Apollo 16 microbreccia (67075,48). The two high relief grains in the field centre are of coarsely exsolved pyroxenes (see Fig. 2 for enlargement of the larger grain). Most of the material shown is of brecciated calcic plagioclase feldspar, feldspathic clasts and dark, finely crushed areas.

plagioclase (An96 to An97). The exsolved pyroxene fragments are distributed evenly, both within and outside the shadowy and ill-defined clasts. The total pyroxene content is only about 4% by volume, but the textural characteristic (Fig. 1) is found in more than 50 detectable grains. Olivine occurs as rounded grains with dark, cloudy reaction rims, the composition being uniform at Fo44. Rare opaque minerals include ilmenite and chrome-spinel, both with low $Mg/(Mg+Fe)$ of 0.06. The total non-feldspathic component is less than 8% by volume.

The commonest pyroxene size is 200–300 μm , each pyroxene being an angular crystal without evidence for marginal remelting or reaction with the enclosing matrix. The example given in Fig. 1 shows characteristic lamellae orientated on the (001) plane of the host crystal which shows cleavage traces parallel to the *c*-axis. The larger grain is shown enlarged and under cross-polars in Fig. 2. There, the host pyroxene has additional (100) exsolution, parallel to the cleavage traces. Another crystal (Fig. 3) shows, in addition to coarse exsolution on (001), fine-scale exsolution on (001) within the pyroxene phases. These two examples are typical of most grains. An additional variety, however, occurs as an orthopyroxene host with rare, irregular blebs of higher birefringence clinopyroxene. All examples are strongly reminiscent of subsolidus exsolution textures within pyroxenes from terrestrial layered basic intrusions, such as the Skaergaard and Bushveld.

The compositions, in terms of the principal element variables (electron probe analysis), are plotted on Fig. 4. Each pair of analysed points, joined by a tie-line, represents the alternating coarse lamellae within a single grain. In most cases (Fig. 3) it is not possible to distinguish between host and lamella, but the compositional nature of the lamellae was characterized in each case, and a modal count made to determine the bulk composition of each grain before exsolution. This method needs refinement, so the semi-quantitative bulk compositions are not plotted on Fig. 4.

Two features of special interest emerge from Fig. 4. First, the alternating lamellae have been equilibrated at subsolidus temperatures to a remarkable extent. Both the Ca rich and Ca poor phases plot at nearly constant calcium

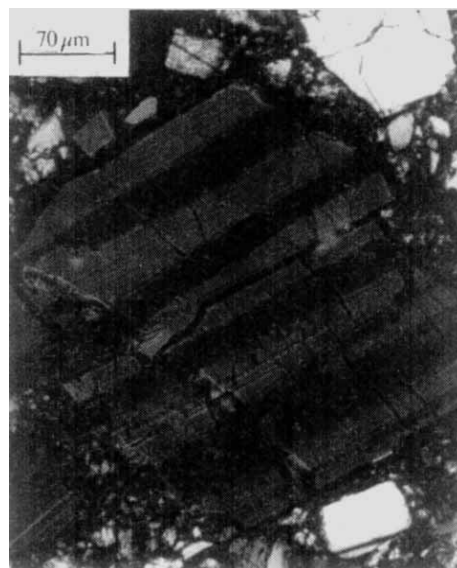


Fig. 3 Photomicrograph, under crossed polars, of a subcalcic augite (bulk composition) with coarsely exsolved calcium rich and calcium poor phases (the compositions of the two phases are the most Mg rich pair on Fig. 1; see also Table 1, analyses 1A and 1B). Note also the fine-scale exsolution parallel to the coarser (001) lamellae. Sample 67075,43.

contents, the compositional ranges paralleling the joins diopside-hedenbergite and enstatite-ferrosilite (Ca43 and Ca2, respectively, in Fig. 4). The lengths of each tie-line must, therefore, approximately define the gap between the two solvus limbs within this system at the lowest temperature of equilibration. For comparison, two exsolved pyroxenes from the Bushveld intrusion¹ are shown on Fig. 4. One pair represents an exsolved augite with clinohypersthene lamellae and the other an exsolved pigeonite inverted to hypersthene with augite lamellae. Both phases occur in the same gabbroic rock and are believed to represent extreme equilibration during very slow cooling of this vast intrusive body. The temperatures are difficult to estimate in the

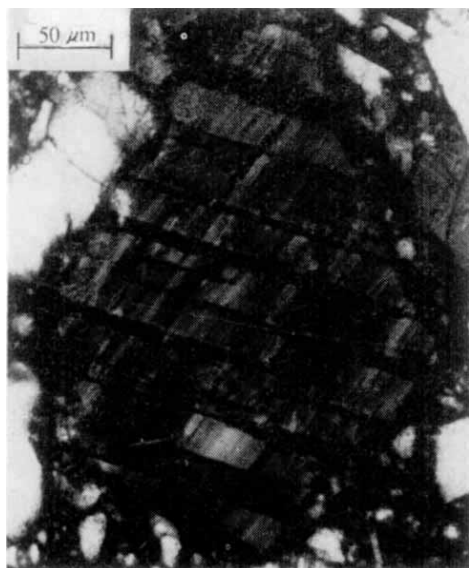


Fig. 2 Photomicrograph, under crossed polars, of calcium poor pyroxene with coarse lamellae of calcium rich pyroxene (the compositions of the two phases are the most Fe rich pair on Fig. 4). In this case a pigeonite crystal exsolved augite on the (001) plane became clinohypersthene by re-equilibration with the lamellae on further subsolidus cooling, and exsolved more calcic pyroxene as the fine lamellae on (100), parallel to the visible (110) cleavage traces. Sample 67075,48.

Table 1 Electron Probe Analyses of Pyroxene Exsolution Lamellae from Sample 67075

	1A	1B	2A	2B	3A	3B
SiO ₂	53.34	52.52	51.33	51.55	50.44	51.49
TiO ₂	0.11	0.17	0.23	0.39	0.26	0.57
Al ₂ O ₃	0.52	0.73	0.44	0.80	0.20	0.86
FeO	21.34	9.68	27.34	12.44	32.12	14.73
MnO	0.38	0.14	0.55	0.30	0.60	0.39
MgO	22.96	15.27	18.39	13.00	14.70	11.27
CaO	1.14	22.14	1.15	20.71	0.98	20.40
Cr ₂ O ₃	0.21	0.26	0.23	0.24	0.22	0.33
Total	100.00	100.91	99.66	99.43	99.52	100.04
Unit formulae:						
Si	1.980	1.950	1.974	1.962	1.988	1.967
Ti	0.003	0.005	0.007	0.011	0.008	0.016
Al	0.023	0.032	0.020	0.036	0.009	0.039
Fe	0.663	0.301	0.879	0.396	1.059	0.471
Mn	0.012	0.004	0.018	0.010	0.020	0.013
Mg	1.271	0.845	1.054	0.738	0.864	0.642
Ca	0.045	0.881	0.047	0.845	0.041	0.835
Cr	0.006	0.008	0.007	0.007	0.007	0.010
O	6.000	6.000	6.000	6.000	6.000	6.000
Atomic ratios:						
Fe	33.49	14.83	44.39	20.02	53.91	24.17
Mg	64.22	41.70	53.22	37.29	43.98	32.96
Ca	2.29	43.46	2.39	42.70	2.11	42.88

Paired sets are selected as relatively Mg rich (1A–1B), intermediate (2A–2B) and Fe rich (3A–3B), as shown in Fig. 1.

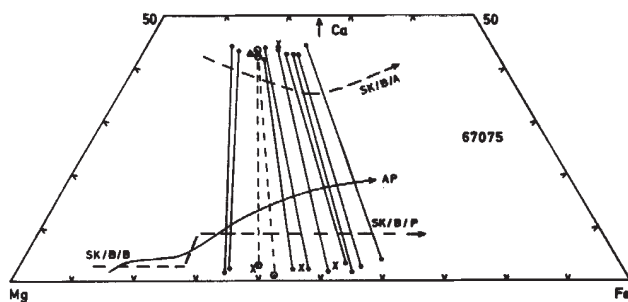


Fig. 4 Plot of the Ca-Mg-Fe contents of analysed, subsolidus-equilibrated pyroxenes in the quadrilateral $\text{CaMgSi}_2\text{O}_6$ - $\text{CaFeSi}_2\text{O}_6$ - $\text{Mg}_2\text{Si}_2\text{O}_6$ - $\text{Fe}_2\text{Si}_2\text{O}_6$. The solidus assemblage trends for comparable pyroxenes are shown for Skaergaard and Bushveld augites (SK/B/A), bronzites (SK/B/B) and pigeonites (SK/B/P), and for lunar Apollo pigeonites (AP). ●—●, Lamellar pairs; ×, one-phase fragments; △, inclusion in plagioclase; ○, terrestrial (Bushveld) lamellar pairs.

absence of experimental data within the pyroxene quadrilateral, but "annealing" at about 900° C would be a likely estimate.

A second feature is the wide range in Mg:Fe ratios between pairs (Fig. 4). The orientation of the tie-lines provides a crude estimate of the distribution coefficient (K_D) for Mg and Fe^{2+} between coexisting Ca rich and Ca poor pyroxenes³. It has usually been assumed that the value of K_D is a guide to the temperature of equilibration between paired compositions, lower temperatures leading to successively decreasing values. This would be represented graphically by a rotation of the tie-lines (Fig. 4) in an anticlockwise direction with decreasing temperatures of equilibration*. Instead, the subsolidus assemblages plot on similar tie-lines to the solidus pairs¹, suggesting that subsolidus re-equilibration is achieved by calcium redistribution between the exsolved phases and is not accompanied by observable Mg and Fe^{2+} redistribution. So the K_D values for the Apollo 16 pyroxene assemblage do not provide the desired guide to temperatures of equilibration. Instead, they give values of about 0.6–0.7 which are identical to those of the Bushveld exsolved assemblage, but also to the Bushveld higher-temperature solidus assemblage². The most dependable evidence for re-equilibration (Fig. 4) includes the trends for solidus assemblages from terrestrial augites and pigeonites; the extent of subsolidus re-equilibration can be gauged from the displacement of the plotted points, giving "stable" trends with constant Ca values for all the sets of pairs.

It is important to remember that the plotted pairs on Fig. 4 are for subsolidus lamellae. The semi-quantitative estimates of bulk compositions chiefly indicate, first, augite bulk compositions, exsolved to a calcic augite host with (001) lamellae of clinohypersthene; and second, pigeonite bulk compositions, exsolved to a clinohypersthene host with (101) lamellae of augite. The principles for very similar terrestrial occurrences are discussed elsewhere^{1,3}.

The chief point is that an original assemblage of Ca rich and Ca poor pyroxenes, varying widely in Mg:Fe²⁺ ratios, has been re-equilibrated at subsolidus temperatures in either a plutonic or a metamorphic environment. The Ca poor subsolidus assemblage is displaced far from the general trend of lunar Ca poor pyroxenes (AP, Fig. 4) and the same is true of the Ca rich assemblage. But lunar basalt pyroxenes show complex solidus compositional relations within the pyroxene quadrilateral, and it is just possible that the assemblage we are considering is of metamorphosed crystals with strongly contrasted bulk compositions, derived from fragmented, zoned basaltic lava pyroxenes. Lack of support for this lava-derivation hypothesis comes from the minor element analyses. Three examples of pairs, given in

* Lengths of tie-lines and Mg:Fe ratios, however, complicate this over-simplified concept.

Table 1, show the usual type of equilibrated distribution¹ of Al, Ti, Cr and Mn between the Ca rich and Ca poor lamellae. They also show, however, extremely low values for Al and Ti in both types of lamellae (and hence in bulk compositions) compared with any of the thousands of lunar pyroxene analyses now available (Fig. 5).

A metamorphic process would be more conventional, in terms of present ideas on the microbreccias having been annealed in hot ash flows at 600° C or higher. The difficulties in accepting such a hypothesis are, however, appreciable. The wide range in pyroxene Mg:Fe ratios within a single slice of breccia would require that the source crystals were strongly zoned on a coarse scale, and fragmented roughly parallel to zonal interfaces. The processes of fragmentation (by meteoritic impact) and annealing would be separate events, for otherwise the annealing would have homogenized the zoned crystals before producing the subsolidus exsolution. Finally, the pyroxenes so far analysed from lunar basalts, microbreccias or soils do not show the low Al contents of this assemblage, and a unique source is therefore to be sought.

The simplest explanation for the observation would be to draw an analogy with strongly fractionated, terrestrial layered intrusions. There the Ca rich and Ca poor pyroxenes show a regular decrease in Mg:Fe²⁺ ratios with lowering solidus temperatures. They also show subsolidus exsolution identical in textures (although coarser) and compositional effects to those shown in the lunar crystals. The disruption of a fractionated plutonic complex, situated within the lunar crust, is therefore a reasonable possibility. Plagioclase was probably a co-precipitating phase because small pyroxenes within plagioclase crystals show the low Al, as well as the subsolidus equilibration effect, shown by the other grains (Figs. 4, 5). The ferri-olivines could have coexisted with the more ferri-olivine pyroxenes shown in Fig. 4, the more magnesium pyroxenes having been precipitated from tholeiitic type liquids with which more magnesium olivines were unstable.

The microbreccia is from an ancient highlands formation on the Moon, so the pyroxene fragments must have been derived from an even older Pre-Imbrian complex. The pyroxenes are unlikely to have been derived directly from lunar mantle pyroxenite material, in view of their wide range in Mg:Fe ratios. Compelling evidence for localized ancient plutonic complexes on the Moon lies in the existence of granitic rocks in the ancient breccias, and of troctolitic fragments with ultra-basic compositions. We believe that

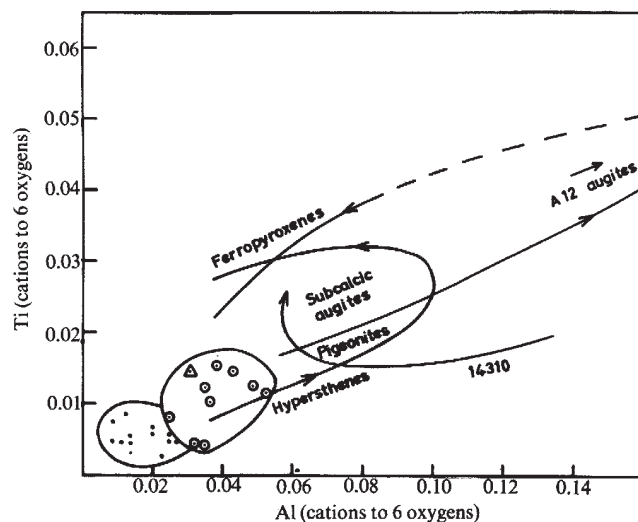


Fig. 5 Plot showing the low aluminium and titanium contents of the analysed pyroxenes, compared with generalized trends for other lunar pyroxenes, including those from a feldspathic lunar basalt (14310). ●, Ca poor lamellae; ○, Ca rich lamellae; △, Ca rich crystal in plagioclase.

much remains to be learned about the early pre-mare magmatic history of the Moon from a study of exotic mafic fragments from both recent and earlier mission collections.

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The Thickness of Deep Seismic Zones

SEISMIC ray tracing¹⁻⁴ has shown that the slabs of lithosphere which descend into the mantle in subduction zones are approximately 80 km thick. Within these slabs there are thin zones in which earthquakes occur⁵. Here I estimate the thickness of the seismic zone in Tonga as ~ 11 km, from the source dimensions of the large intermediate earthquakes.

From hypocentre locations, the thickness of the seismic zone has been estimated to be at most 25 km in the Tonga area^{6,7} and approximately 10 km in Southern Alaska⁸ and the Central Aleutians⁹. Because the hypocentre determinations are still affected by considerable errors it has been suggested^{6,7} that the deep earthquake zone may be as thin as 5 km.

Hypocentre locations give an upper limit for the thickness of the seismic zone, but source dimension estimates for individual earthquakes provide a lower limit. The whole of the failure area of an earthquake must be contained within the seismically active part of the slab. The minimum estimate is possible because the two conjugate fault planes typically cut across the brittle zone at 45° angles¹⁰ (Fig. 1). Thus the width of either of the two planes perpendicular to the slab is equal to the thickness of the seismic zone divided by $\cos(45^\circ)$. In the direction parallel to the strike of the slab the fault plane could be elongated, but spectral observations show that except for the largest earthquakes the source areas are approximately circular¹⁵. From first motion studies it is evident that all earthquakes can be modelled as double couple sources. Fukao¹⁰ and Abe¹¹ have made strong cases for two deep earthquakes to have been dislocations on a plane. I therefore take the focal mechanism of non-shallow earthquakes to be a failure on a plane.

For four earthquakes for which the source area was known from surface faulting and aftershock locations, the source dimensions have been estimated from seismic spectra^{12,13}. The dimensions derived from spectra exceeded the field observations by 8 to 25%. If this comparison is also valid for deep and intermediate earthquakes the source dimensions of deep earthquakes can be derived with accuracies of approximately 30%. Because the source dimensions are about 10 to 20 km the proposed method to determine the thickness of the brittle zone is competitive with the hypocentre locations. In some oceanic areas where near stations cannot be deployed the accuracy of hypocentre locations may never be better than 10 km. In these areas the proposed method may remain the most accurate way of estimating the thickness of the deep seismic zone.

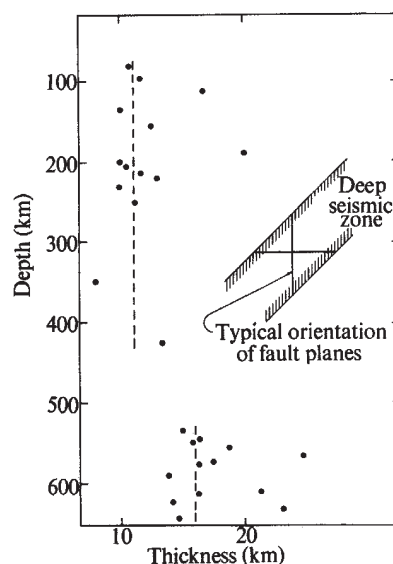


Fig. 1 Thickness of deep seismic zone in Tonga. ●, Estimates based on source dimensions of individual earthquakes¹⁵. Inset shows geometry of typical fault planes.

In the Tonga-Kermadec and other regions it was found that the source dimensions of earthquakes with $m_b \geq 5.6$ were fairly constant for focal depths between 70 and 700 km (refs. 14, 15). This surprising result, together with the constant width of the hypocentre scatter as a function of depth, suggests that the limitation on the source dimensions is imposed by the thickness of the deep seismic zone and that the large earthquakes extend across the whole width of the deep seismic zone. In that case the source dimensions provide not a minimum but a true estimate of the thickness of the brittle zone. The lack of extensive aftershocks at depth also supports this idea and may be partly explained by a model where the failure extends across the seismic zone, ending on either side at what may be considered a free surface because the material beyond it will deform by creep in response to stress. Aftershocks would still be expected in directions parallel to the strike of the deep seismic zone.

I therefore suggest that the source dimensions of magnitude 6 earthquakes indicate the thickness of the deep brittle zones. Fukao¹⁰ has proposed that the largest earthquakes break across the slab. I believe that the few gigantic deep earthquakes which have approximately double the source dimensions of all the other large events¹⁵ may be explained by especially thick portions of the brittle zone, or by fault planes more nearly parallel to the dip of the zone, as is the case for the event studied by Abe¹¹.

Fig. 1 shows the estimated thickness of the brittle zone in the Tonga as a function of depth. The thickness of the deep seismic zone is 11 km at intermediate depths. This value is close to the thickness of the seismic zone under Southern Alaska⁸. Around 600 km depth the seismic zone in Tonga is distorted⁷ and the source dimensions indicate a thickness of 16 km.

The lithosphere under the oceans is estimated to be 70 km thick¹⁶. The brittle part is again much thinner and confined to the very top layers. At the ridge crest where the crust is produced, the brittle part is thinner than 5 km, which is approximately the cutoff depth for hypocentres in Iceland²⁰. Source depths on the San Andreas Fault and slip-rate comparisons on other transform faults¹⁷ indicate that the brittle crust is between 1 and 10 km thick at transform faults some distance away from the spreading centre. Below the Tonga trench the thickness increases to 11 km in the straight part of the slab and 16 km in the bent and possibly buckled part at great depth. This suggests that the thickening of the

brittle crust occurs fairly rapidly near the ridges, which is in agreement with the estimated geothermal gradients there¹⁸.

It seems that farther away from the ridge crest the brittle crust remains constant in thickness over great distances.

I conclude that the size of deep and shallow earthquakes is limited by the thickness of the brittle crust. Very large earthquakes are only possible on planes dipping nearly like the brittle crust, that is, in the mega thrusts of subduction zones.

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Deposition of Sulphur Dioxide on Grass

SULPHUR dioxide may be removed from the atmosphere in rain, by oxidation resulting in particulate material, or by direct interaction of the gas molecules with the lower boundary of the atmosphere. The last mechanism is known as dry deposition and is independent of the nature and condition of the surface. This communication reports the first results of a programme of measurements of the dry deposition of sulphur dioxide studied by two separate methods.

In the first method the variation of concentration χ of sulphur dioxide with height z is measured. Because of deposition at the surface and eddy diffusion in the atmosphere the concentration decreases toward the surface and the flux is given by

$$F = K(z)d\chi/dz \quad (1)$$

where $K(z)$ is the eddy diffusivity for sulphur dioxide. Thus F can be obtained from simultaneous measurements of $K(z)$ and $d\chi/dz$. We assume that the eddy diffusivity for sulphur dioxide approximates that for momentum, K_m . K_m can be estimated from the profiles of wind speed and temperature provided that measurements are made close to a large horizontal uniform surface where the constant-stress assumption applies. The site used was a grass field on chalky soil offering a fetch of 150 or 200 m in the upwind direction.

Sulphur dioxide concentrations were measured at heights of 0.2, 0.5 and 1.0 and 2.0 m. Air was drawn at 30 l. min⁻¹ for

periods of 60 to 120 min through sampling trains comprising, first, heated polystyrene (microsorban) filters which remove particles but not sulphur dioxide and, second, glass bubblers containing hydrogen peroxide solution.

The sulphur dioxide trapped in the bubblers was determined as sulphate by the method due to Persson¹. The wind speed was measured at five heights from 0.25 to 2 m using cup-anemometers and the air temperature was measured up to 4 m with platinum resistance thermometers.

The weather was cloudy and buoyancy effects were small in most cases, so the wind speed $u(z)$ was fitted graphically to

$$u(z) = u_* \ln((z-d)/z_0)/k$$

where d is the zero displacement, z_0 the roughness length, u_* the friction velocity and k Von Karman's constant. The eddy diffusivity $K_m(z)$ at 1 m could then be calculated because

$$K_m(z) = k.u_*(z-d)$$

In two cases when the heat flux was not negligible a correction (of the order of 10%) was derived by calculating the Richardson number and using the results of Businger².

The sulphur dioxide concentration was also plotted as a function of height to obtain the gradient of concentration at 1 m so that the flux F could be calculated using equation (1) and the deposition velocity $v_g = F/\chi(1)$ obtained.

In the second method a small amount ($\sim 10^{-3}$ mol) of sulphur dioxide containing 30 mCi of ³⁵S was released at a height of 1 m at a position 100 m upwind of the sampling section, a marked area near the centre of the field. The concentration in air was measured above the crop using peroxide bubblers from which an aliquot was taken for liquid scintillation counting. The flux of ³⁵SO₂ to the crop was measured by sampling several measured areas of the crop. The ³⁵S content was determined by extraction of total sulphate and liquid scintillation counting, and the deposition velocity was calculated from $v_g = F/\chi(1)$.

Simultaneous measurements of the wind speed profile enabled the roughness length z_0 and friction velocity u_* to be determined.

The results, presented in Table 1, show the deposition velocity for each measurement. Also shown is the resistance for deposition, $r = 1/v_g$. The resistance may be considered as the sum of resistances acting in series:

$$r(z) = r_a(z) + r_s$$

where r_a is the aerodynamic resistance and r_s the surface resistance. The latter reflects any departure of the surface from perfect sink behaviour and must depend on the nature and condition of the surface. We can calculate r_a from the wind profile allowing for the difference between mass and momentum transfer using the sublayer Stanton number of Owen and Thomson³. An estimate of this parameter for sulphur dioxide was made from the work of Chamberlain⁴ and Thom⁵. The calculated value of r_a was subtracted from r to determine r_s (Table 1).

The surface resistance, r_s , is generally a significant part of r showing that the surface cannot be considered a perfect sink. The values of r_s obtained by the two methods cover a similar range, varying by more than an order of magnitude, but no obvious relationship between r_s and the prevailing conditions emerged. In view of the suggestion of Cowling and Jones⁶ that sulphur dioxide fulfils an important role compensating for sulphur deficiency in the soil, and because of its significance as an air pollutant, it is of interest to consider the long term deposition rates implied by the results. Using the average of the fifteen determinations, $v_g = 0.0085$ m s⁻¹, a mean concentration of 40 μ g m⁻³ (ref. 7) in country districts would yield a deposition rate of 110 kg of sulphur dioxide per hectare year or, if a further extrapolation is justified, 1.9 Mton yr⁻¹ over the country as a whole. This is nearly 40% of the annual produc-

Table 1 Measurements of the Deposition Velocity of Sulphur Dioxide on Grass

Date	Concentration $\chi(1) \mu\text{g m}^{-3}$	u_* m s $^{-1}$	$u(1)$ m s $^{-1}$	z_0 m	Mean v_g m s $^{-1}$	Error	r s m $^{-1}$	r_s s m $^{-1}$
Radioactive tracer method								
June 8, 1972		0.63	4.8	0.040	0.022	± 0.0005	45	20
July 17, 1972		0.40	4.5	0.010	0.0036	± 0.0003	280	230
August 29, 1972		0.40	4.1	0.014	0.0094	± 0.0018	106	66
September 14, 1972		0.37	3.8	0.024	0.017	± 0.002	59	11
October 18, 1972		0.38	3.8	0.026	0.0076	± 0.0007	130	76
November 2, 1972		0.27	2.7	0.014	0.012	± 0.0014	83	20
Gradient method								
November 22, 1972	12	0.29	3.3	0.0105	0.0079		130	65
November 22, 1972	21	0.21	2.3	0.0115	0.0069		150	64
December 13, 1972	36	0.29	3.3	0.0105	0.0086		110	53
December 21, 1972	34	0.053	0.60	0.012	0.0016		610	330
December 21, 1972	71	0.084	0.85	0.012	0.0025		400	220
January 11, 1973	20	0.25	2.7	0.012	0.0093		110	37
January 11, 1973	14	0.30	3.4	0.009	0.013		77	15
January 12, 1973	20	0.24	2.6	0.012	0.012		86	14
January 12, 1973	26	0.24	3.0	0.006	0.0073		140	58

tion of 5 Mton and more than twice the 0.7 Mton yr $^{-1}$ deposited by rain.

It is evidently desirable to extend the measurements reported here to a wide variety of surfaces, so that the influence of dry deposition on the long distance travel of sulphur dioxide may be determined. It is already clear, however, that dry deposition is an important mechanism for the removal of the gas from the atmosphere.

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Generation of Chemical Waves

A RECENT communication on the generation of spatial waves by the Zhabotinskii reaction¹, although reporting interesting experimental results, unfortunately propagates a serious error originally perpetrated by Busse². This implies that periodic variations in space are possible for a simple linear autocatalytic reaction when diffusion of the autocatalytic species is taken into account. The steady state form of the equation in question is

$$D \frac{d^2 c}{dz^2} + Kac = 0 \quad (1)$$

where c is the concentration of autocatalytic species C , and a is the concentration (assumed constant) of another reactant A . The general solution of this equation is

$$c = C_0 \sin(2\pi z/\lambda + \phi) \quad (2)$$

where C_0 and ϕ are arbitrary constants and

$$\lambda = 2\pi(D/Ka)^{1/2}$$

In both refs. 1 and 2 boundary conditions are ignored; but

it is obvious that a sink for C (non-zero gradient) must be postulated at the walls of the container if a steady solution is to be possible, in order to compensate for the autocatalytic production of C within the volume. Otherwise there would be an explosion. In fact equation (2) has long been known as representing the free radical concentration in a chain branching system with heterogeneous wall termination³. Bursian and Sorokin³ did not, however, interpret the sinusoidal form of the solution as representing waves of any kind because they realized that the only physically acceptable solution is one which is positive throughout the container, if it is to represent a concentration. Thus equation (2) is a physically acceptable solution only when the container dimension is less than or equal to half a wavelength; otherwise negative concentrations intrude. The sinusoidal solution then simply represents a distributed concentration for the species C and has no connexion with chemical waves at all. The same criticism applies to Rastogi and Yadava¹ except that they have transformed (incorrectly incidentally) into moving coordinates.

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Crystallographic Evidence for Ade-AdeH Hydrogen-bonded Pairs in a Cobalt-Adenine Complex

INTEREST in the role of metal ions in the biochemical reactions of nucleotides and nucleic acids¹ has led us to study the isolation of solid metal complexes with nucleotides and their bases, and to perform X-ray structural studies on suitably crystalline materials. In this way unequivocal information can be obtained concerning the preferred coordination sites of a given metal ion and also concerning the important question of hydrogen-bonding interactions in these systems. We report here the isolation of a crystalline complex of cobalt (II) sulphate with adenine (Ade) and details of its crystal structure.

Pale pink needles of composition $[\text{Co}(\text{H}_2\text{O})_4(\text{Ade})_2](\text{AdeH})_2(\text{SO}_4)_2 \cdot 6\text{H}_2\text{O}$ (where AdeH represents the protonated adeninium cation $\text{C}_5\text{H}_6\text{N}_5^+$) were obtained from aqueous solutions of adenine and cobalt (II) sulphate in 2:1 mol ratio. (Analytical results: Found, C, 24.4; H, 3.8; N, 28.7; S, 6.6; calculated, C, 24.65; H, 4.35; N, 28.8; S, 6.6%.) The crystals are monoclinic, with unit cell dimensions $a = 13.971$, $b = 7.190$, $c = 19.900$ Å, $\beta = 101.82^\circ$, $U = 1956.6$ Å³, space group $P2_1/n$ and $Z = 2$. The structure has been determined on the basis of 2,275 independent reflexions measured on a Siemens four-circle diffractometer using Cu-K α radiation. Least-squares refinement has now reached $R = 0.065$.

The structure contains the crystallographically centrosymmetric $[\text{Co}(\text{H}_2\text{O})_4(\text{Ade})_2]^{2+}$ ion, AdeH and sulphate ions, and molecules of water of solvation, all connected by an intricate network of hydrogen bonds. In the complex cation (Fig. 1) the cobalt atom has an octahedral coordination and the coordinated adenine is monodentate, bonding *via* N(9), as in $[\text{Cu}(\text{AdeH})_2\text{Br}_2]\text{Br}_2$ (refs. 2, 3) while in $\text{Zn}(\text{AdeH})\text{Cl}_3$ (ref. 4) it bonds *via* N(7). In the other three metal-adenine complexes whose structures have been determined hitherto, the inner complex $[\text{Cu}(\text{C}_5\text{H}_4\text{N}_5)_2\text{H}_2\text{O}]_2 \cdot 6\text{H}_2\text{O}$ (ref. 5), $[\text{Cu}(\text{Ade})_2\text{Cl}_2] \cdot 2\text{H}_2\text{O}$ (refs. 6, 7), and $\text{Cu}_3\text{Cl}_8(\text{AdeH})_2 \cdot 4\text{H}_2\text{O}$ (refs. 8, 9), adenine acts as a bridging ligand bonding *via* N(3) and N(9).

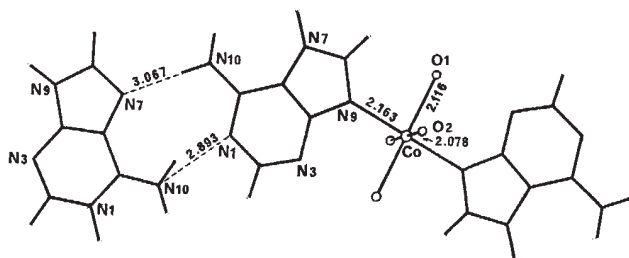


Fig. 1 A fragment of the structure showing the centrosymmetric $[\text{Co}(\text{H}_2\text{O})_4(\text{Ade})_2]^{2+}$ ion and a hydrogen-bonded AdeH group. Standard deviations are: Co-O, 0.005; Co-N, 0.005; and $\text{N} \cdots \text{N}$, 0.009 Å.

We have been able to locate unequivocally the positions of the hydrogen atoms for both the Ade and AdeH moieties, and have indicated them in Fig. 1. The hydrogen bond network found in this structure contains the following types of bond: $\text{O}-\text{H} \cdots \text{O}$, $\text{N}-\text{H} \cdots \text{O}$, $\text{O}-\text{H} \cdots \text{N}$, $\text{N}-\text{H} \cdots \text{N}$, and possibly $\text{C}-\text{H} \cdots \text{O}$. The full structural details will be published elsewhere.

Another feature of this structure which is of some biological interest is the presence of hydrogen-bonded pairs Ade-AdeH oriented as shown in Fig. 1. Of the two $\text{N}-\text{H} \cdots \text{N}$ hydrogen bonds joining the pair $\text{N}(10)-\text{H} \cdots \text{N}(1)$ is the shorter one. A similar connexion of Ade units is found in 9-methyladenine¹⁰ and in deoxyadenosine monohydrate¹¹. These, however, form an infinite zigzag chain whereas in this structure the interaction is limited to a discrete pair because of protonation at the crucial N(1) site in AdeH.

Although caution should be exercised in making an extrapolation to complex biological systems, it is conceivable that if under certain conditions an individual adenine base in a nucleic acid becomes protonated, then this may help to stabilize an abnormal adenine-adenine pairing. The rare, even transient, formation of such a linkage followed by normal replication provides one simple mechanism for a mutation, where in this case Thy is substituted for Ade in the DNA sequence. This abnormal pairing may also occur in the transcription stages involving RNA, the end product in each case being a single erroneous amino-acid in the peptide sequence or an aberration in the chain termination process.

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On Boiling an Egg

Dwek and Navon¹ give the temperature of boiling water on the top of Pike's peak as 91° C; at an altitude of 14,110 foot the atmospheric pressure should correspond to a boiling point of 86° C. I can, however, confirm their observations from experimental results obtained at 7,300 foot (93° C boiling point); these indicate that an egg may be hard-boiled in 12 minutes. My results support these authors' contention that considerably less than 12 h is required, either at 91° or 86° C, to hard-boil an egg.

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BIOLOGICAL SCIENCES

Isolation of the Islets of Langerhans for Transplantation

PANCREATIC transplantation with the aim of treating diabetes mellitus has so far met with little success. Of 23 patients thus treated and reported to the Transplant Registry in 1971¹, 15 died within 3 months and the longest survived one year. One of the major problems has been to overcome pancreatic exocrine digestion, and pancreatic duct ligation ("Banting pancreas") before transplantation has been performed. It was shown by Dragstedt², however, that dogs treated in this way often became diabetic or showed a diabetic glucose tolerance test after several months, probably due to fibrosis and consequent ischaemia of the Islets. Transplantation of the whole

gland with its vascular supply is a major undertaking and the problems of thrombosis, leakage and digestion, coupled with immunological rejection, have prevented success so far.

Attempts have been made to isolate the Islets of Langerhans from the pancreas in order to study glucose metabolism. A micro-dissection technique was described by Hellerstrom³ but this was tedious and produced only small numbers of Islet clumps. Subsequently Moskalewski⁴ described a method of collagenase digestion which was successful in the rabbit and improved the yield, and Lacy and Kostianovski⁵ modified the method for the rat. There were still difficulties in harvesting the liberated Islets, however, even with methods of separation such as zonal centrifugation and density gradients, and there were also problems with viability of the cells.

We have developed a technique by which large numbers of Islets of Langerhans are prepared consistently from the rat pancreas for purposes of transplantation. Viability was confirmed by transplantation.

Initially, albino outbred rats and subsequently young adult inbred hooded rats (strain PVG/C) were used. Following killing by cervical dislocation, the ventral surface of the animal was shaved, prepared with chlorhexidine in spirit and the abdomen opened with a midline incision. Using a dissecting microscope, magnification $\times 10$, a fine polyethylene catheter ('Intracath'—B. R. Bard, London) was introduced into the common bile duct and secured with a 2-0 linen thread ligature. The lower end of the duct was occluded with an artery forceps just before its entry into the duodenum. The pancreas was distended by injection of 10 ml. Hanks' solution containing bovine albumen (fraction V), 2 mg ml.⁻¹. It was found that after some practice this procedure could be completed within 5 min of the death of the animal. The pancreas was removed, transferred to a glass Petri dish, cut into small pieces with scissors and any excess fat removed. It was then transferred to a tube to which a further 10 ml. of Hanks' solution was added. The pancreatic tissue sank to the bottom and any remaining fat floated on the surface and was readily aspirated and discarded. The prepared tissue was transferred to a small conical flask together with 2 ml. Hanks' solution containing glucose 0.6 mg ml.⁻¹ and collagenase type 1 (Sigma Chemicals, London). The stoppered flask was placed in a shaking water bath at 37° C for about 30 min. The exact time for separation was determined by frequent sampling and examination under the dissecting microscope.

The digested pancreas was transferred to a tube and diluted with further cold Hanks' solution containing glucose in the same concentration as before and gently centrifuged for 1 min. The supernatant was discarded and fresh medium added, and the resultant suspension filtered into a Petri dish with a blackened base for examination under the dissecting microscope at $\times 10$ magnification.

Initially the view was obscured by fine fragments of acinar

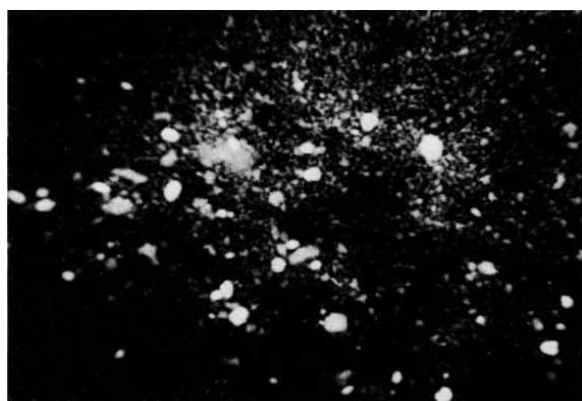


Fig. 1 Islet cell tissue seen after isolation from the pancreas using dark ground illumination. Dissecting microscope, $\times 5$.

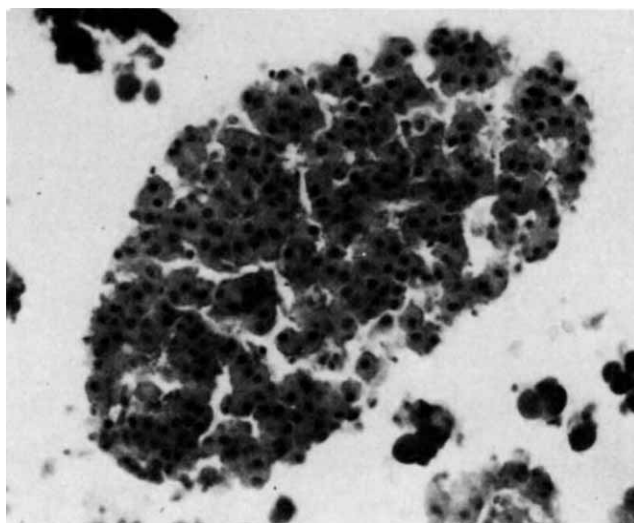


Fig. 2 Isolated pancreatic Islet. Normal architecture and appearance of cells. Haematoxylin and eosin, $\times 300$.

tissue, which were removed by gently agitating the Petri dish and allowing them to become suspended in the Hanks' solution, then aspirated and discarded, the Islets remaining on the bottom of the vessel. They could then be seen clearly (Fig. 1) by the aid of dark ground illumination as yellowish white domes and were picked out with a finely drawn Pasteur pipette. Fig. 2 shows the histological appearance of an isolated pancreatic Islet. Its architecture and cells appear normal.

Yields of up to 350 Islets per rat pancreas have been achieved using this method. To obtain larger quantities, rat pancreases have been processed in batches of four.

Viability of the isolated cells was confirmed by transplantation beneath the renal capsule and into the testis of isogenic rats. The longest period of follow-up was one month, when viable looking Islet cells containing beta cell granules staining with aldehyde-fuchsin were seen. A similar method has been successfully applied in the rabbit.

The relationship of the Islets of Langerhans to diabetes mellitus was established in 1889 by Von Mering and Minkowski⁶, and in 1892 Hedon⁷ demonstrated that subcutaneous implantation of a small piece of pancreas could delay the appearance of diabetes in an animal that had undergone pancreatectomy. The concept of Islet cell grafting appears to be neglected in the extensive literature on pancreatic transplantation, although one early report suggests that transplanted Islets may modify alloxan diabetes in the rat⁸.

We have now established a successful method of isolation of Islets of Langerhans in an animal strain in which inbred immunologically isogenic lines are available, making possible transplantation studies uncomplicated by rejection problems. These cells can be grafted and survive for appreciable periods of time as shown by the viability studies. Histological and functional studies of Islet cell transplantation will be reported elsewhere.

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Remarks on Virus-like Particles in Human Breast Cancer

MANY investigators have tried to correlate the genesis of human breast cancer with the presence of a virus. In spite of many electron microscopical studies^{1,2}, however, no convincing evidence of an association of human breast cancer with virus has been demonstrated. Last year, Moore and his colleagues³ reported that they had found particles with the same morphological characteristics as those that cause breast cancer in mice (B particles), in the milk of 5% of the American women with no history of the disease in their families; in 60% of the Americans with a history of the disease in their families; and in 39% of Parsi women. Sarkar and Moore⁴ distinguish the particles in human milk with a size and shape similar to the B particles into three classes: MS-1 identical to the B particles; MS-2 particles with the surface projections different from B, and MS-3 smooth particles. These results encouraged us to examine milk samples from Dutch women for the presence of virus-like particles.

Because our method of isolation of B particles from mice with mammary tumour virus (MTV)⁵ is different from the method used by Moore *et al.*⁶, the following preliminary experiments were performed in order to decide which isolation technique would be used. Small amounts of milk (0.05–1 ml.) from MTV infected BALB/c mice were added to 8-ml. samples of human milk. Each sample was divided into two equal parts. One part was treated according to the method of Moore and his coworkers and put on a two-step sucrose gradient (52% and 42%) followed by centrifugation on a continuous gradient of (5%–25%) Ficoll in phosphate buffered saline (PBS). Sometimes the centrifugation through Ficoll was omitted. The other half was treated according to our method and centrifuged on a four-step gradient of Ficoll and D₂O (Fig. 1a); as a buffer 0.01 M Tris (pH 7.0) + 0.25 M sucrose was used. In solutions with

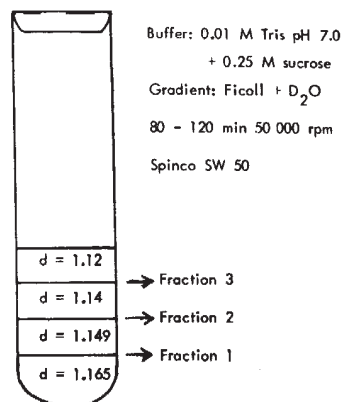
this sucrose concentration the infectivity of C particles is well preserved during sedimentation⁷ and in our own experience the morphology of both B and C particles is better preserved than in PBS or other buffers. The use of Ficoll and D₂O as a gradient for isopycnic centrifugation has the advantage that the gradient is not hypertonic compared to the buffer system and thus an osmotic shock of the particles, during preparation, is avoided. The different fractions of each treatment were collected by puncturing the bottom of the tubes. Part of the material was pelleted and prepared for ultrathin sectioning; the rest was used for negative staining.

In three different experiments the added mouse B particles could easily be recognized in the material from the Ficoll/D₂O gradient both in negatively stained preparations (Fig. 1b) and in thin sections (Fig. 1c), even after a 1:80 dilution with human milk. This corresponds to the amount of virus in 25 μ l. of mouse milk. Following the method of Moore *et al.*⁶, we had more difficulties in finding mouse B particles in preparations after similar dilutions.

Because the preliminary test had shown that our own isolation method did not damage the structure of the B particles and may have the additional advantage of preventing an osmotic shock, this method was used in the following experiments. We examined forty-two milk samples (3–10 ml.) from women with no cancer history and one sample from a woman with a family history of breast cancer. After negative staining we always found three different types of structures in fractions 1 and 2 (Fig. 1a): (a) roughly spherical or elongated particles of different size that sometimes exhibited a fringe of fine projections ranging from 45 to 71 Å (Fig. 2a); (b) roughly spherical particles with an irregular electron dense inner structure and a thin periphery (Fig. 2b); (c) particles with a head and a tail that sometimes carried surface projections (like MS-2) (Fig. 2c). In fraction 3 (Fig. 1a), we found many smooth particles with a head and a tail that sometimes showed blebs (like MS-3) (Fig. 3).

The MS-3 particles found in large quantities in fraction 3 resemble negatively stained C particles. It is difficult to identify MS-3 as C particles for the following reasons: Milk is the product of an apocrine gland and may therefore contain cytoplasmic debris. Part of this debris (cell membranes) may assume, in negatively stained preparations, shapes with tail-like appendages similar to the C particles^{8,9}. With the purification methods used not only all particles with the same density as B or C particles were isolated, but also, because of the limited centrifugation time, all particles with about the same size. For these reasons we correlated

Fig. 1 a, Schematic representation of partial purification for particles from human milk by isopycnic centrifugation on a Ficoll/D₂O gradient. b and c, Fractions of human milk to which mammary tumour virus-containing mouse milk has been added. b, A B particle after negative staining showing the typical spikes (arrow) ($\times 168,000$). c, Thin section of a B particle (arrow) with an excentric nucleoid surrounded by a membrane ($\times 64,000$).



a

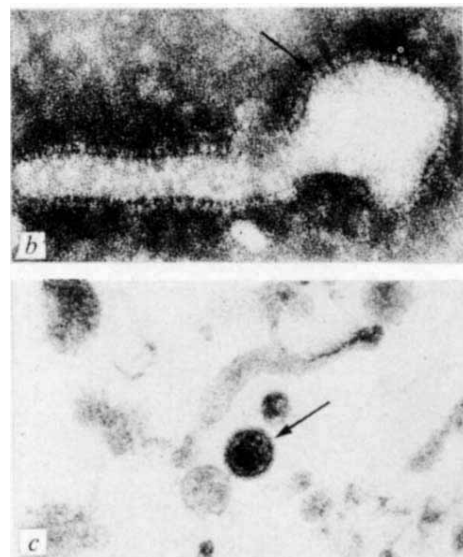


Fig. 2 Particles present in fractions 1 and 2 from human milk after negative staining. *a*, Particle with surface projections. *b*, Particle with an irregular electron dense structure that resembles the nucleoid of the oncornaviruses and with a smooth membrane (arrow). *c*, Particle with head, tail and remnants of broken projections (p) and the membrane (m). The head is broken at one side. ($\times 225,000$.)

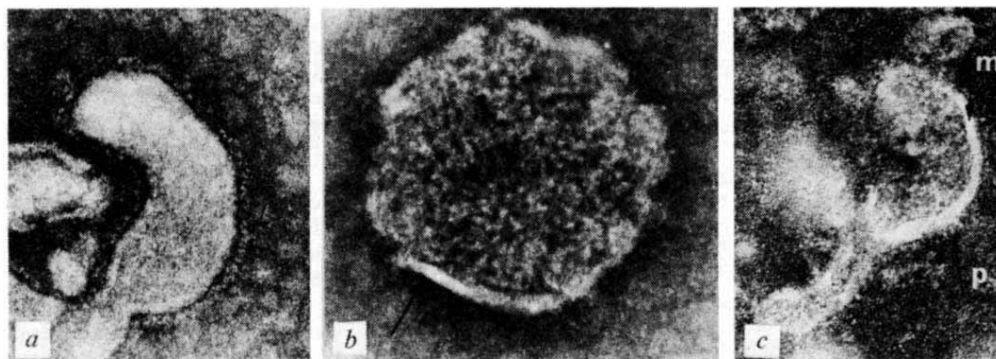
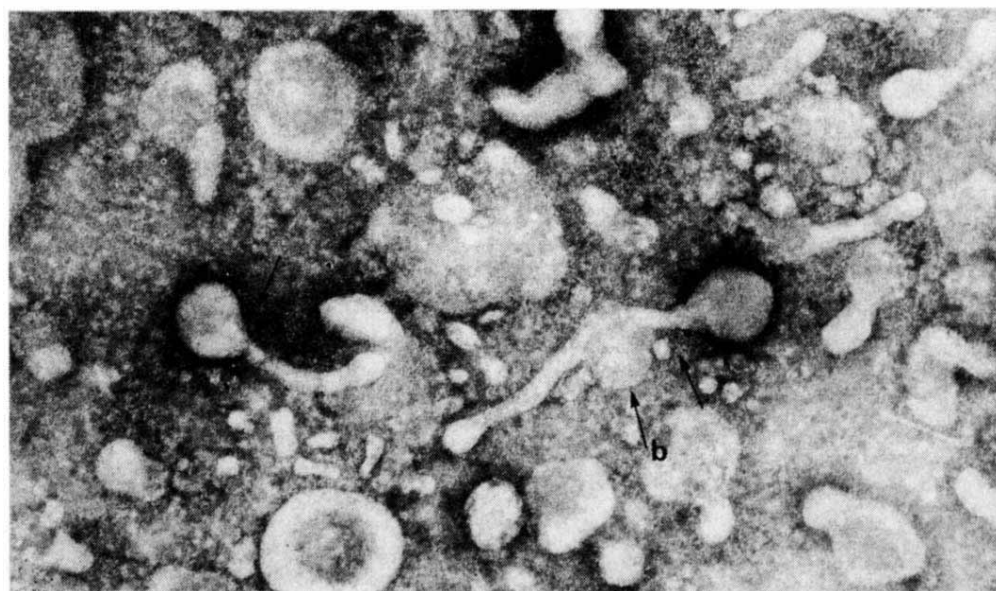


Fig. 3 Representative field of fraction 3 from human milk after negative staining. Two particles (arrows) resembling MS-3 type particles are shown; in one a bleb (b) is visible. ($\times 105,000$.)



our results in negatively stained preparations with the study of the same material viewed in thin sections. If C particles were present it should be possible to identify them in thin sections by their nucleoid. However, no virus-like particles, neither of the B-type nor of the C-type, were found in thin sections.

Both in negatively stained material and in thin sections of the partially purified milk samples of forty-three women, the observed particles resemble those found by Jensen and Schidrowsky¹⁰ in normal bovine and human milk. This indicates that these particles are present very frequently regardless of the source or type of milk, and makes it likely that they are cytoplasmic debris or secreted products from the mammary gland.

Sarkar and Moore¹¹ also reported five experiments in which five different human milk samples were mixed with mouse milk containing B particles. In three cases they found that the human milk altered the morphology of the B particles. We did the same experiment with six different human milk samples that were mixed, before removing the cream, with milk from MTV infected BALB/c mice. We were unable to find any damaging effect of human milk on the structure of B particles after incubation of the mixture at 37° C for 1 h. Only when the mixture was kept overnight at 4° C or 37° C did the amount of broken B particles increase, but this happened also in the control experiment when mouse milk from MTV infected BALB/c had been mixed with mouse milk from MTV-free BALB/c and the mixture was kept overnight at 4° C or 37° C. In several other experiments on MTV many B particles were damaged when mouse milk was kept overnight at 4° C.

Our investigation of the partly purified milk samples from

43 Dutch women has shown that: (1) no intact B particles nor intact C particles were found; (2) B particles from mouse milk were not morphologically influenced by substances from human milk in the cases reported here; (3) the different types of particles that were observed are probably cytoplasmic debris and secreted products from the mammary gland.

A human mammary tumour virus might not have the stability or the same morphology as the MTV of mice. Although both electron microscopical (compare Fig. 1*a*³ and Fig. 3*a*⁴) and biochemical indications¹² exist for the presence of such a virus in human milk, one must be careful in correlating⁴ the presence of rather unidentified particles like MS-2 and MS-3 in human milk with other characteristics like presence of RNA-dependent DNA polymerase or the breast cancer history of the family of the donor.

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Congenital Heart Disease and Maternal Smoking Habits

FEDRICK *et al.*¹ have produced data to show that the incidence of congenital heart disease in infants of mothers who are cigarette smokers is significantly higher than among infants

X-rays. Furthermore, at the end of the study all suspected children were examined and all records were reviewed by one paediatric cardiologist (Dr Julien I. E. Hoffman, University of California Medical Center, San Francisco). We were therefore confident that the final diagnoses were valid, and that the mothers' accounts of their smoking history were not biased by the outcome of their pregnancies.

The incidence of congenital heart disease among the infants of smokers was almost identical in our study and in that of Fedrick *et al.* (7.3% in their study, which is identical to the incidence among all races in our study, and very close to the 8.2% we obtained for whites). But we found a much greater incidence of congenital heart disease among the children of non-smokers (8.1 and 7.7 for all races and for whites, respectively, compared with 4.7).

After a thorough search of the literature, consisting of eighteen studies, four based on relatively large samples^{7,8}, I conclude that the findings of Fedrick *et al.* are at variance with most previous investigations, especially those based on relatively large samples (Table 2). Thus, contrary to the statement¹ that, "It is generally accepted that maternal smoking during pregnancy is associated with an increased risk of spontaneous abortion, low birth weight and stillbirth or neonatal death", the mortality rate is not greater for infants of smokers, and the perinatal mortality rate of low birth weight infants of smoking mothers is significantly lower than that of low birth weight infants of non-smoking mothers.

Table 1 Proportion of Smoking and Non-smoking Mothers of Children with and without Congenital Heart Disease (CHD)

Maternal smoking habits	Congenital heart disease				Live births without CHD			
	All races		Whites		All races		Whites	
	No.	%	No.	%	No.	%	No.	%
Smokers	38	33.0	31	39.7	5,158	35.3	3,735	38.1
Non-smokers	77	67.0	47	60.3	9,458	64.7	6,077	61.9
Total	115	100.0	78	100.0	14,616	100.0	9,812	100.0

Table 2 Perinatal Mortality* Among all Infants and Among Low-Birth-Weight Infants by Maternal Smoking Status in Four Studies of Large Sample

Study		Number of births	% low-birth-weight (≤2,500 g)		Total Non-smoker	Perinatal mortality per 1,000		
			Non-smoker	Smoker		Smoker	≤2,500 g	
							Non-smoker	Smoker
Yerushalmy ²	White	5,381	3.5	6.4	12.4	13.9	232.1	137.7
	Black	1,419	4.9	13.4	23.4	22.9	260.9	109.4
Underwood <i>et al.</i> ³		48,505	5.7	8.9	19.7	20.8	269.0	187.0
Rantakallio ⁴		11,931	3.5	6.1	23.2	23.4	343.6	287.6
Butler <i>et al.</i> ⁵		16,994	5.4	9.3	32.4	44.8	284.5	268.5
Yerushalmy ⁶	White	9,793	3.2	6.4	11.0	11.3	218.3	113.9
	Black	3,290	5.8	12.3	17.1	21.5	201.6	113.6

*Study of Yerushalmy refers to neonatal mortality only. A more complete review of all the evidence is given in the correspondence published in refs. 7 and 8.

of non-smoking mothers. In our child health development studies my colleagues and I have attempted to duplicate those findings which relate to their "control week" data. We used the same criteria, thus omitting all multiple births and all cases in which the defect was associated with anencephalus, spina bifida or Down's syndrome. We found no difference in the proportion of smokers and non-smokers between mothers of affected and unaffected children. Thus we found no difference between the children of smoking and non-smoking mothers with regard to the incidence of congenital heart disease (Table 1).

Our study was entirely prospective. The information on smoking was derived early in pregnancy, whereas in the study by Fedrick *et al.* the questionnaire was completed after delivery. Also, the children in our study were observed continuously and any suspected of congenital heart disease were given all appropriate tests, including electrocardiogram and

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Reply to Professor Yerushalmy's letter on Congenital Heart Disease

We were greatly interested in Professor Yerushalmy's data, for it is only by collecting information from many different sources that a finding can be substantiated epidemiologically.

Obviously any study in which, for example, the smoking history was obtained during pregnancy rather than after delivery is at an advantage. We believe, however, that the type of bias inherent in any retrospective survey is unlikely to have occurred to any marked extent in the present series because at the time of the inquiry there had been no publicity concerning the effects of smoking in pregnancy.

It is difficult to find any explanation (other than that due to chance) of the differences between the incidences of congenital heart disease (CHD) in the two populations of births. We would like to point out, though, that in our paper we had a second series of 204 cases of CHD among 7,000 perinatal deaths, and that the differences in incidence of CHD between infants of smokers and non-smokers were of the same order of magnitude as in our population series, even when variables such as maternal age, parity and social class had been allowed for.

As to the effect of smoking on perinatal mortality in general, we are somewhat surprised at Yerushalmy's interpretation of his Table 2 in which he states that the perinatal mortality rate in general is not larger for infants of mothers who smoke. In fact, of the selected studies he reports, and excluding Yerushalmy's 1964 sample which is a subsample of the 1971 sample, three studies show a very small increase in perinatal mortality and two studies show a large increase. Furthermore, he fails to mention the fact that all these studies give a similar estimate (150–200 g) for the average reduction in birthweight associated with smoking in pregnancy. Other studies¹ have shown that the reduction in gestation is only marginal, and we conclude that the effect of smoking is to produce a degree of foetal growth retardation. It is, however, irrelevant to compare the mortality rates of all infants under 2,500 g. Growth retardation although carrying a high risk of perinatal death is entirely different from low birth weight due to immaturity. The latter carries an even higher perinatal mortality death rate, and must obviously be considered separately.

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¹ Butler, N. R., Goldstein, H., and Ross, E. M., *Brit. Med. J.*, **2**, 127 (1972).

Marginal Vitamin B₁₂ Intake during Gestation in the Rat has Long Term Effects on the Offspring

It is now evident that malnutrition of the foetus or of the infant results in long term abnormalities in the physical, clinical and behavioural characteristics of the organism^{1–4}. Experimental studies have generally dealt with the effects of severe malnutrition. There is reason to believe that less severe dietary imbalances, more likely to occur in population groups of the Western world, if imposed at an early stage, may have profound, adverse effects much later in life. Because the nutrient demands per unit of body weight are presumably

higher in the foetus than in the newborn, a diet which is considered fully adequate for post-natal development does not necessarily provide, when consumed by the mother, an adequate nutritional environment for the developing foetus.

In previous studies⁵ we observed that a high dietary level of B₁₂ resulted in increased weight of the newborn compared with that of offspring from mothers receiving a basal diet in which the B₁₂ level was somewhat in excess of the estimated needs for pregnant rats⁶. This latter dietary B₁₂ level supports a normal or adequate birth weight in rats and permits a rate of post-natal growth which is considered to be within acceptable limits. However, based on the birth weight differences, our results suggested that the level of B₁₂ in the basal diet represented a marginal deficiency of the vitamin and so we have explored its long term effects on the wellbeing of the offspring.

Pregnant rats received a dietary B₁₂ intake (50 µg/kg diet) which somewhat exceeded the estimated requirement for the pregnant, lactating and growing rat⁶, but which approximated a concentration often used in purified diets, or a level in considerable excess of this intake (50,000 µg/kg diet). Female rats (Sprague-Dawley, Charles River Laboratories) were housed in screen-bottom cages, given water *ad lib*. and fed from weanling on a basal diet. This diet contained (percentage by weight): 20.0 C-1 assay soybean protein; 28.9 glucose; 28.5 sucrose; cotton seed oil 15; choline 0.3; DL-methionine 0.3; salt mix 5.0; vitamin mix 2.0. Further details of diet have been described⁷. A supplement of crystalline B₁₂ was added to the diet, at the time it was mixed, to achieve a concentration of 50 µg/kg of diet. At maturity, the female rats were placed in breeding units, three animals per cage. They continued to receive the basal, (control) diet and were bred to normal male rats of the same strain. Breeding was confirmed by a positive vaginal sperm smear. At this time, the female rats were transferred to individual cages and they were randomly allocated to receive the basal diet or the higher supplemental vitamin B₁₂ level (50 × 10³ µg/kg diet). Mean food intake during the 21-day gestation period was essentially the same in both groups: 454 g and 461 g for the basal and high B₁₂ groups, respectively. Similarly, mean total weight gain during this period was 135 g and 128 g for the basal and high B₁₂ groups, respectively. Two days after birth, litter size was reduced to eight and the mother was then fed the control diet until the young were weaned at 21 days. After weaning, the progeny continued to receive basal diet until they were used for various measurements. Although the higher levels of dietary B₁₂ were given only during gestation it is recognized that the B₁₂ supplied to the offspring born to mothers receiving the higher supplement of B₁₂ most probably remained higher during lactation than those born to mothers given the control diet. Offspring were taken at various periods during the first year of life for assay of liver aminopyrine demethylase and glucose-6-phosphatase (EC3139) activity⁸.

The latter was also visualized by histochemical means in both liver and kidney⁹. Enzyme assays were conducted on the livers of year-old rats which were fasted overnight and tissues collected for histological assessment¹⁰. To assess the possible clinical benefit gained from the increased B₁₂ intake we utilized an experimental model previously described for study of the metabolic effects of a bacterial infection¹¹. Rats were infected by oral administration with 5 × 10⁹ organisms of *Salmonella typhimurium* at 3 months of age. Liver levels of B₁₂ were determined¹³ in representative female rats at time of conception and at littering and in the newborn for each diet group.

In agreement with our earlier findings, the higher dietary level of B₁₂ during gestation did not affect litter size but significantly increased birth weight (Table 1). Body weight differences were still evident after 1 yr, demonstrating a long term influence of the higher dietary level of B₁₂ provided during intrauterine life. Determinations of total body nitrogen and lipid revealed that the weight difference among the two offspring groups was reflected in a greater proportion of total

Table 1 Mean Values for Body Weight and Liver Enzyme Activity of Offspring born to Mothers receiving Two Levels of Dietary Vitamin B₁₂ and for Liver Concentration of B₁₂ in Dams and their Newborn

Parameter	Group	
	Basal	High B ₁₂
No. of litters	44	57
Litter size	9.7 ± 1.5†	9.9 ± 1.2
Birth weight (g)	5.8 ± 0.2	6.2 ± 0.3†
Body weight§ (g) at 21 days	41 ± 4	51 ± 3†
3 months	351 ± 10	391 ± 11*
1 yr	463 ± 9	510 ± 9†
Liver enzyme activity :		
Glucose-6-phosphatase¶		
4 days	16 ± 2	24 ± 2*
1 yr	60 ± 4	85 ± 6*
Aminopyrine demethylase**		
4 days	28 ± 2	42 ± 3†
1 yr	260 ± 9	340 ± 15†
Liver B ₁₂ concentration: (ng B ₁₂ /g liver)		
Dam††	132 (90–160)‡‡	308 (234–417)
Newborn§§	27 (21–38)	68 (37–94)

*, † Significantly different at $P < 0.05$ and $P < 0.01$, respectively, from the basal group.

‡ Mean values ± s.e.m.

§ Weight of male offspring.

|| Each mean based on twenty animals from ten litters.

¶ Activity expressed as μmol phosphate liberated/min/g, dry, fat-free liver.

** Activity expressed as μg aminoantipyrine formed/h/g, dry, fat-free liver.

†† Mean liver B₁₂ concentration for ten females at time of conception was 173 (range 132–190). Values shown are at time of littering for ten females per diet group.

‡‡ Range of values given in parentheses.

§§ Values based on two animals from each of six litters per diet group.

body protein, as well as liver protein, and a lower proportion of total lipid for rats born to mothers who received the higher level of dietary B₁₂.

Liver B₁₂ levels increased during gestation in the female rats receiving the higher B₁₂ intake and decreased in those given the basal diet (Table 1). These changes were reflected by a higher concentration of liver B₁₂ in the newborn from mothers receiving the high dietary B₁₂ intake. Because total food intake during gestation was the same for each diet group, the differences in liver B₁₂ concentrations and birth weights noted above, among the two diet groups, seem to be causally related to the differences in the dams' B₁₂ intake and not to other dietary factors.

Table 1 also summarizes the *in vitro* activity of the two liver enzymes for the two time periods at which they were examined. Enzyme activity per unit of liver weight and per total liver was higher for rats born to mothers given the higher level of dietary B₁₂. These results may imply a greater capacity for handling foreign compounds entering the liver by way of the portal or peripheral circulation in rats born to mothers receiving the high dietary level of B₁₂.

The results of the *S. typhimurium* infection study are shown in Table 2. Eighteen hours after infection, positive cultures were found in blood samples in about equal numbers for both groups but bacteria were cleared more efficiently from circula-

tion within 48 h in rats born to the mothers receiving the higher supplemental level of vitamin B₁₂. Cumulative mortality for the first 10 days was lower in the offspring of the B₁₂-supplemented group indicating a greater resistance to the experimental infection. This experiment suggests a long term beneficial clinical effect on the offspring of the higher B₁₂ intake during intrauterine development and that they responded more favourably to the stressful stimulus of a systemic infection.

Tissues taken from rats 3 days after infection and processed for histological and histochemical studies revealed a more marked increase in cellularity of the spleen of the high B₁₂ group and a greater concentration of glucose-6-phosphatase in both liver and kidney. These differences are compatible with a more active and responsive metabolic apparatus in rats exposed to higher B₁₂ *in utero*.

The basal level of dietary B₁₂ utilized in these studies is sufficient to achieve a normal birth weight in the newborn, a storage of B₁₂ in the liver at time of birth, and permits a rate of post-natal growth which is considered to be within a normal or acceptable range⁶. However, the findings show that an increased B₁₂ concentration in the maternal diet during the gestation period results in a higher birth weight of the offspring, a higher concentration of liver B₁₂ at birth and, more significantly, in an enhanced capacity of the organism to withstand stress in later life, based on the increased resistance to *Salmonella* infection. The findings suggest that an apparently mild deficiency of B₁₂ may have subtle effects on the differentiation and functional development of the foetus. Furthermore, a marginally deficient nutrient supply at this critical phase in the life of the organism is clinically unremarkable in its effects until some time in later life and then only when the organism is challenged by a stressful stimulus.

These results underscore the potential adverse effects of a marginal maternal nutrient supply on the offspring but cannot be interpreted to mean that intake levels in excess of appropriate dietary standards are of any benefit. Our findings support the conclusions of others¹³, that the effects of maternal nutrition during the prenatal period are carried over into the later life of the offspring, and they also parallel our previous observations¹⁴ on the long term adverse effects of low-lipotrope diets during the gestation period.

We feel that the results of the present studies justify cautious consideration of their implications with regard to the nutrition of the human foetus and breast-fed neonate. Particularly important are those constituents which may be marginal in the mother's diet but without obvious manifestation in herself or of significance for the sibling until either are confronted by an additional stressful stimulus, such as infection and trauma or even during the metabolic changes which occur in response to pregnancy. Questions about many of the unexplained illnesses in children and the wide variation among individuals in their resistance to disease may conceivably be answered by more intensive study of the prenatal nutrient needs of mother and her foetus.

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Table 2 Effect of High Maternal Vitamin B₁₂ during Gestation on the Response of Male Offspring to *Salmonella typhimurium* at 3 Months of Age

Parameter	Group	
	Basal	High B ₁₂
No. rats with positive blood culture:		
18 h after infection	16/20	18/20
48 h after infection	11/20	5/20
10-day cumulative mortality	21/40	9/40

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Absorption, Accretion and Endogenous Faecal Excretion of Calcium by the Newborn Infant

MEASUREMENTS of calcium absorption require the administration of a suitable marker for natural calcium; the most commonly used markers in adults are ^{45}Ca and ^{47}Ca , two radioactive nuclides of calcium which cannot be used experimentally in infants because of the possible risk from radiation. Calcium salts enriched in the non-radioactive nuclides ^{46}Ca and ^{48}Ca are now available and may be used in newborn infants. Non-radioactive nuclides have the advantages that metabolic information can be obtained without exposure of the subject to ionizing radiation and that, if necessary, measurements can be made after prolonged storage of specimens because the marker is not subject to radioactive decay. The absorption, retention and elimination of calcium by the newborn infant are of practical importance¹ and here we provide measurements from which true absorption and endogenous faecal elimination can, for the first time, be inferred.

Four healthy but premature males (ages and weights given in Table 1) were fed from birth and during the tests on a carbohydrate modified cows' milk formula 1610F Ca (ref. 2). A solution containing 2.0 mg of Ca enriched in ^{46}Ca was added to a single normal feed and the intake of milk from this and subsequent feeds throughout the study was measured. Stools, urine and vomitus were collected for a total of 48 h and all specimens, including aliquots of milk, were dried and thermally ashed. The natural calcium content of specimens was measured by atomic absorption spectrophotometry and the ^{46}Ca content was determined from measurements of the radioactive ^{47}Ca produced during neutron irradiation of the specimens. Appropriate corrections were made for naturally occurring ^{46}Ca present in the specimens. Details of this method are given elsewhere³.

We have assumed that the calcium marker and the natural calcium in the milk are equally available for absorption. Although for infants this has not been proved, for adults, the

absorption of ionic and milk calcium is most probably equal when the two are ingested simultaneously⁴.

Because exogenous excretion of the marker had virtually ceased within 24 h of administration, values could be obtained for true absorption, endogenous faecal excretion and accretion of calcium by applying the equations of Aubert *et al.*⁵ to the measurements of natural and marker calcium. The results demonstrate the relatively small variation between four infants in the daily amounts of calcium absorbed (107–171 mg) and accreted (66–118 mg) from the milk used in these tests. The percentage absorption was constant (mean 33.8 ± 1.4), but the endogenous faecal excretion varied widely, from 0 to 20% of the intake. The difference in endogenous excretion between different infants may prove to be relevant to the aetiology of the calcium deficiency and calcium excess syndromes in the newborn².

Full term infants of the same age range but double the weight, fed on breast milk⁶, ingested $45\text{--}48\text{ mg Ca kg}^{-1}\text{ day}^{-1}$ on average, markedly less than the $217\text{ mg kg}^{-1}\text{ day}^{-1}$ for the premature infants fed on the test formula. Retention in the breast-fed infants was $19\text{--}23\text{ mg Ca kg}^{-1}\text{ day}^{-1}$ as compared with $50\text{ mg kg}^{-1}\text{ day}^{-1}$ for accretion in the premature infants. There is clearly a need for further investigation of the factors, including dietary composition, which affect calcium metabolism in the human infant.

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Selective Synthesis of Liver Nuclear Acidic Proteins following Glucagon Administration *in vivo*

It is known that the metabolism of the non-histone, acidic proteins associated with DNA in the chromatin of animal cells is influenced by steroid hormones. The effects are often highly specific; for example, the administration of hydrocortisone to adrenalectomized rats leads to a selective stimulation of synthesis of a liver chromosomal protein of molecular weight 41,000¹. Hydrocortisone also alters the pattern of phosphorylation of chromosomal proteins in the liver, differential effects becoming evident within minutes after injection of the hormone^{2,3}. Similar specific alterations in the synthesis or phosphorylation of nuclear proteins have been observed in uterine cells responding to oestrogens⁴, in kidney cells following aldosterone administration (C. Liew, personal communication), in prostate cells stimulated by androgens⁵, and in insect chromosomes stimulated by ecdysone⁶. In all these cases, the steroid hormone also affects the RNA synthetic capacity of the target cells, presumably as a result of altered template capacity of the modified chromatin.

Table 1 Intake, True Absorption, Accretion and Endogenous Faecal Excretion of Calcium by Four Premature Infants

Subject	Post natal age days	Weight kg	Mean daily intake mg	True absorption mg/day	% of intake	Endogenous faecal excretion mg/day	Accretion mg/day
L. N.	11	2.3	508	171	33.7	103	66
N. B.	10	1.9	435	161	37.0	46	109
M. M.	9	2.4	480	144	30.0	25	118
L. T.	41	1.5	311	107	34.4	0	105
Mean	18	2.0	434	146	33.8	44	100
and s.e.	(±8)	(±0.2)	(±44)	(±14)	(±1.4)	(±22)	(±12)

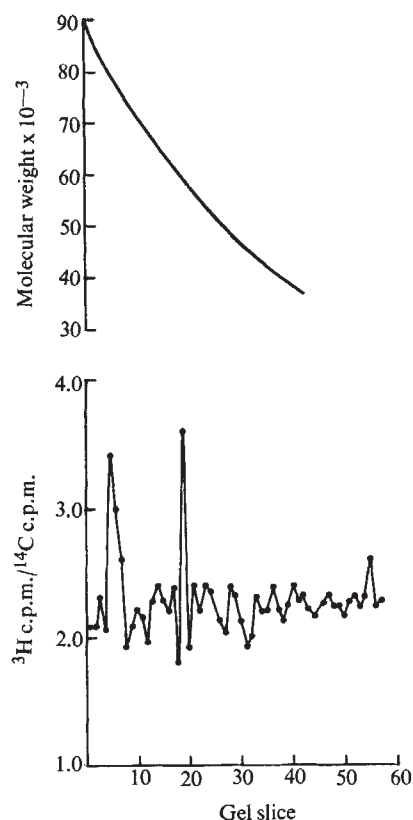


Fig. 1 Electrophoretic separation and radioanalysis of liver nuclear acidic proteins from glucagon-treated and control rats. Matched pairs of rats were injected with glucagon or saline solutions. Hormone-treated animals received 1 mCi of DL-4,5- ^3H -leucine and control animals with 200 μCi of U-L- ^{14}C -leucine 4 h later. After a 1 h labelling period, livers were removed and pooled and the nuclear acidic proteins were prepared by selective extraction of the isolated nuclei in phenol⁹. The mixed proteins from hormone-treated and control animals were co-electrophoresed in 10% polyacrylamide gels containing SDS. The banding pattern was revealed by staining with 'Amidoblack 10B' and the gel was sliced in 1 mm slices. Radioactivity in individual slices was determined by combustion and scintillation spectrometry. The lower curve shows the ratio of $^3\text{H}/^{14}\text{C}$ incorporation into the acidic proteins at different positions in the gel. The upper curve shows the relationship between electrophoretic mobility and protein molecular weight. Note the increased incorporation of ^3H -leucine into proteins of molecular weight 60,000 and 80,000, respectively, in the glucagon-treated rat livers.

To determine whether chromatin composition can also be influenced by peptide hormones, we have investigated the effects of glucagon on the synthesis of different acidic proteins of liver nuclei under conditions in which gene activation for RNA synthesis is known to occur. Gene activation by glucagon is manifest in the *de novo* synthesis of a number of liver enzymes, the induction of which depends on the synthesis of "new" RNA molecules^{7,8}.

To compare the synthesis of chromosomal proteins in control and hormone-treated animals, pairs of adult, male Sprague-Dawley rats, maintained on 'Purina' chow diet *ad libitum*, were injected intraperitoneally with 400 μg of glucagon in 0.5 ml. of 0.14 M NaCl. Control animals received isotonic saline. After 4 h, the hormone-treated animals were pulse-labelled for 1 h by intraperitoneal injection of 1 mCi of DL-4,5- ^3H -leucine (specific activity 15 mCi per mmol). Control animals received 200 μCi of uniformly-labelled L- ^{14}C -leucine (specific activity 300 μCi per mmol) at the same time.

After the labelling period the animals were killed by cervical dislocation and the livers were removed and pooled before isolation of the acidic chromosomal protein fraction. This procedure ensured that the ^3H -labelled proteins from the glucagon-treated animals and the ^{14}C -labelled proteins

from the control animals were exposed to the same conditions throughout the course of their isolation.

The livers were minced and homogenized in 0.32 M sucrose and 3 mM MgCl_2 and the nuclei were purified by centrifugation through 2.4 M sucrose and 1 mM MgCl_2 as described previously⁹. The isolation of the nuclear acidic proteins was based on their solubility in phenol^{1,9} after removal of the saline-soluble proteins in 0.14 M NaCl and extraction of the histones in 0.25 M HCl. The acid-extracted nuclei were treated to remove lipids and subsequently suspended in 0.1 M Tris (HCl), pH 8.4, containing 0.14 M 2-mercaptoethanol and 0.01 M EDTA. The suspension was extracted with phenol and the protein in the phenol phase was subsequently restored to an aqueous phase by a series of dialyses against urea-containing buffers⁹. The acidic chromosomal proteins were finally dialysed against 0.01 M sodium phosphate buffer, pH 7.4, containing 0.14 M 2-mercaptoethanol and 0.1% sodium dodecylsulphate (SDS). Aliquots of this solution containing 250 μg of protein were analysed by electrophoresis in 10% polyacrylamide gels. The electrophoretic separation of the acidic protein-SDS complexes was carried out as described previously⁹. The gels were stained with 'Amidoblack 10B' to reveal the characteristic banding pattern of the liver nuclear phenol-soluble proteins, and cut transversely in 1 mm slices. The radioactivity in each slice was determined by scintillation spectrometry after separation of the ^3H -labelled and ^{14}C -labelled combustion products in a 'Packard Tri-Carb Model 305 Sample Oxidizer'.

The results of the double-labelling experiments are summarized in Fig. 1. The electrophoretic pattern contained a series of protein bands, each of which contained a mixture of the ^3H -protein from the hormone-stimulated nuclei and the corresponding ^{14}C -protein from the control nuclei. In Fig. 1, the ratio of ^3H -activity to ^{14}C -activity is plotted for each of the 1 mm slices cut from the gel. This ratio is an indication of the relative rates of synthesis of individual nuclear proteins derived from the glucagon-treated and control animals. The $^3\text{H}/^{14}\text{C}$ ratio was remarkably uniform for all proteins in the gel except for two bands located in gel slices 5 and 19. Here the abrupt change in isotope ratio indicated a selective stimulation of synthesis of these particular proteins in the glucagon-treated animals. The relationship between protein mobility and molecular weight is plotted in the uppermost curve of Fig. 1. The molecular weights of the proteins specifically responding to glucagon stimulation were estimated at approximately 60,000 and 80,000 respectively.

It is of interest that no change in isotopic amino-acid ratio was evident in the protein band corresponding to a molecular weight of 41,000 which was shown to be responsive to cortisol stimulation¹. This is a clear indication of the different modes of action of the steroid and peptide hormones on the nuclei of the same target tissue.

The question arises as to whether the effects of glucagon on the synthesis of liver nuclear proteins are direct, or whether they are mediated through the release of other hormones, such as insulin or pituitary hormones. The effects of insulin were examined in parallel studies in which adult, male rats weighing about 200 g were injected with 1.25 units of insulin, and control rats with saline. Hormone-treated animals were pulse-labelled 4 h later, for 1 h with ^3H -leucine, and control animals received ^{14}C -leucine. The livers were pooled, and the nuclear acidic proteins were prepared and analysed by polyacrylamide gel electrophoresis as described. The $^3\text{H}/^{14}\text{C}$ ratio was determined for each of the mixed protein bands in the gel, and no significant increase in ratio was observed for those protein bands which were most responsive to glucagon administration. This result makes it unlikely that the glucagon effects on nuclear protein metabolism are mediated through the release of insulin.

This is the first indication that peptide hormones such as glucagon can modify the composition of the chromatin by selectively stimulating the synthesis of particular nuclear acidic proteins. There is extensive evidence, however, that cyclic AMP mediates the action of glucagon on the liver¹⁰⁻¹² and that it also has a rapid and selective effect on the phosphorylation of different acidic proteins of the liver nucleus¹³.

Administration of glucagon *in vivo* leads to a selective stimulation of synthesis of two acidic proteins in liver cell nuclei, of molecular weight 60,000 and 80,000 daltons, respectively.

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Are African Elephants deficient in Essential Fatty Acids?

THE African elephant (*Loxodonta africana*), living in its natural habitat in East Africa, has suffered over the last few decades from progressive restrictions resulting in overcrowding within national parks and sanctuaries^{1,2} and a relatively rapid deterioration of its habitat from woodland to open grassland^{3,4}. Much of the latter change has been precipitated by the elephants themselves pushing over young trees to eat the succulent growing shoots, or stripping the oil bark and exposing the trees to damage by periodic fires⁵.

Laws⁶ described puberty retardation and an extension of the mean calving interval in mature cows in northern Uganda elephant populations. He suggested that these changes were due to inadequate nutrition. Sikes⁷ has also suggested the presence of a nutritional stress in grassland elephants. Intake of the essential fatty acids (EFA), linoleic and linolenic, by grassland elephants decreases dramatically during the dry season⁸, paralleled by a similar decrease in the growth rate⁹. Wild African buffaloes (*Syncerus caffer*), living in deteriorated habitats similar to those occupied by elephants, have been shown to have lower concentrations of essential fatty acids in their tissues than buffaloes living in more wooded areas of East Africa¹⁰, and a similar pattern of depletion might therefore occur in grassland elephant populations.

I investigated this possible EFA deficiency by analysing the component fatty acids of the lipids extracted from the sera obtained from two adult female non-pregnant lactating elephants, shot in Murchison Falls National Park, Uganda. Serum collection and the mean serum lipid values of these and other elephants have been reported elsewhere¹¹. Separation of the major lipid classes was carried out by thin-layer chromatography and the fatty acid methyl esters from each class were prepared by *trans*-methylation¹². The component fatty acid esters were examined by gas-liquid chromatography, using a flame ionization detector, and two columns of differing polarity (polyethylene glycol adipate on celite at 180°, and 'Apiezon L' on celite at 200°). Peaks were identified by comparison of their log retention times with those of methyl palmitate on both columns, and by comparison with standard mixtures.

Table 1 Fatty Acid Composition (wt. % of Total) of Serum Cholesterol Esters and Triglycerides

Fatty acid	Cholesterol esters		Triglycerides	
	Murchison Falls elephant serum*	Kenya-Uganda elephant serum†	Murchison Falls elephant serum*	Kenya-Uganda elephant serum†
14:0	0.4	1.0	3.2	2.6
16:0	15.5	14.0	43.1	40.5
16:1	5.5	2.8	2.2	2.6
17:0	0.4	0.3	0.7	1.4
17:0 br	—	0.8	—	1.0
18:0	1.8	1.8	8.6	10.2
18:1	35.2	21.2	38.8	31.9
18:2	29.7	42.4	0.6	5.2
18:3	7.6	1.3	2.8	0.2
20:3	0.2	5.2	—	0.9
20:4	3.7	5.6	—	0.9

* Mean of 2 ♀ elephants from Murchison Falls National Park, South Bank.

† Mean of 2 ♀ and 3 ♂ elephants from Kenya and S.W. Uganda; Moore and Sikes¹³.

Table 1 shows the percentage fatty acid composition (by weight) of the serum cholesterol esters and triglycerides. Each figure represents the mean of the two individual samples. Also included in the table for comparison are the values obtained independently by Moore and Sikes¹³ for elephants from a different East African population. The cholesterol in the serum of the elephant was predominantly esterified with unsaturated fatty acids, as it is in man¹⁴. The major saturated fatty acid was palmitic acid (16:0), as in man¹⁴, the domestic ox¹⁵, and the rabbit¹⁶. In these species, the major unsaturated fatty acid in the cholesterol esters is linoleic acid (18:2) but in the elephant samples it was oleic acid (18:1). This is in contrast to the findings of Moore and Sikes (see Table 1) who found twice as much linoleic acid as oleic acid in the cholesterol esters of elephant plasma, but the animals they studied were from populations which had access to plentiful browse, unlike the animals described here. These investigators also found 5.2% (see Table 1) of an eicosatrienoic acid in the cholesterol esters which they identified as 20:3, *n*-6. Neither of the two elephants described here had appreciable amounts of this fatty acid in its serum cholesterol esters.

Table 2 records the fatty acid composition of the serum phospholipids from these elephants. As these animals were both lactating adult females, I have included in the table for comparison the data for serum phosphoglyceride composition for adult women given by Olegard and Svennerholm¹⁷ as well as the additional elephant data of Moore and Sikes¹³. It will be seen that in the elephant the predominant fatty acid was stearic (18:0) whereas in women it is palmitic (16:0); also both these analyses and those of Moore and Sikes showed considerably less linoleic acid (18:2) in the phosphoglycerides of the elephant than in women. As with the cholesterol esters and triglycerides, the elephants described here had a

Table 2 Fatty Acid Composition (wt. % of Total) of Serum Phospholipids from Two African Elephants

Fatty acid	Phospholipid composition		
	Serum from Murchison Falls elephants*	Serum from Kenya-Uganda elephants†	Plasma from post-parturient women‡
14:0	0.2	—	—
16:0	13.5	15.4	31.3
16:1	0.2	0.9	1.4
17:0	0.2	0.8	—
18:0	41.7	32.6	11.5
18:1	15.5	12.2	14.3
18:2	9.1	12.3	22.1
18:3	2.0	0.7	1.0
20:0	1.1	1.4	—
20:2	3.3	1.2	—
20:3	4.3	6.2	3.3
20:4	4.4	6.3	8.5
22:0	4.5	—	—

* Mean of 2 ♀ elephants from Murchison Falls National Park, South Bank.

† Mean of 2 ♀ and 3 ♂ from Kenya and S.W. Uganda; Moore and Sikes¹³.

‡ Mean of 20 Swedish women; Olegard and Svennerholm¹⁷.

lower concentration of linoleic acid in the phospholipids than those described by Moore and Sikes.

Appreciable amounts of eicosatrienoic fatty acids were present in the phospholipids of these elephants' serum. There are at least two different eicosatrienoic acids which may occur in serum lipids, one synthesized from oleic acid (18:1, *n*-9) in EFA deficient animals¹⁸ and designated 20:3 *n*-9, and another which is an intermediate in the synthesis of arachidonic acid (20:4, *n*-6) from linoleic acid (18:2, *n*-6) and which is designated 20:3, *n*-6. The value shown in Table 2 for phospholipid 20:3 is a combined figure for total eicosatrienoic acids. A ratio of total eicosatrienoic acid to arachidonic acid in the phospholipids in excess of 0.4 has been shown by Holman¹⁹ to be characteristic of EFA deficiency. While Holman suggests that this ratio should be more precisely stated as 20:3, *n*-9/20:3, *n*-6, he also suggests that the triene-tetraene ratio is valid in most natural dietary situations in which linoleate is the dominant EFA. I did not separate 20:3, *n*-6, from 20:3, *n*-9, and therefore cannot state this ratio exactly, but the dominant fatty acid in the dietary lipids of the elephant is certainly linoleate⁸ and I feel justified in estimating this ratio from total 20:3 and 20:4 levels, more especially as there is evidence from recent studies that 20:3, *n*-6, is raised as well as 20:3, *n*-9, in experimental EFA deficiency^{20,21}.

The total eicosatrienoic:arachidonic acid ratios in the phospholipids of the two elephants studied here were 0.57 and 1.04 respectively. Both these ratios are in excess of 0.4, which suggests that their dietary intake of EFA was inadequate. This interpretation is strengthened by a re-examination of previous analyses of the diet. These showed that fat represented only 4.6% of the total calorific value of the elephants' diet and that linoleate represented only 4.7–9.5% of the total dietary fatty acids. It can be calculated from these figures that linoleic acid accounted for only 0.2–0.4% of the total dietary calories, well below the 1% level necessary to prevent EFA deficiency in rats, guinea-pigs, swine and man²².

If EFA deficiency exists in the Murchison Falls elephant population, as this work suggests, it could also exist in other overcrowded populations in East Africa, and thus go a long way towards explaining the remarkable tree damage inflicted by elephants in Serengeti National Park in Tanzania, Tsavo National Park in Kenya, as well as in Murchison Falls National Park. The Terminalia trees in Murchison Falls and Baobab trees (*Adansonia digitata*) in Tsavo are both highly sought after by elephants. The woody pulp and bark from these trees have been found to contain higher concentrations of linoleic acid than other available grazing in these areas

(ref. 8, and unpublished observations). Excessive tree damage by elephants may therefore be a natural response to an inadequate fatty acid intake.

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Lightning as Background Noise for Communication among Electric Fish

AN appreciation of the background noise of the communication "channel" is crucial to our understanding of animal communication systems. Lightning¹⁻⁴ is one source of naturally-occurring, non-biological electrical noise that is within the frequency range of electric communication by fish (below 20 kHz), and there may be others⁵. It is an important noise source because thunderstorms are especially common over South America and Africa where electric fish are found^{6,7}, and electromagnetic waves from lightning propagate long distances according to the inverse first power of distance¹. I investigated the importance of this noise during a field study of gymnotid fishes in Guyana, South America (3° 19' N; 59° 39' W).

On June 21, 1971, at 2300, I immersed two small wire electrodes, 30 cm and 50 cm apart, to a depth of 15 cm in a quiet region of a small, freshwater creek. The specific conductance of water was 4.36×10^{-5} mho cm⁻¹. The electrodes were oriented horizontally and were connected by shielded wires to an audio amplifier and tape recorder ('Uher 4400'). The overall frequency response (3 db) of the recording system was from 20 Hz to 18 KHz and the RMS thermal noise of the system with the input shorted was less than 2×10^{-8} V Hz^{-1/2} across the frequency spectrum, significantly less than the signals to be discussed here.

Oscilloscope tracings of recorded electrical activity revealed many brief fluctuations in the electric field of the order of several microvolts per cm. There were often ringing trains of

oscillations typical of the multiple echoes between the Earth and ionosphere (Fig. 1a). Sound spectrograms of recorded activity show "clicks" as events composed of many frequencies of nearly equal intensity, and also "tweaks" as frequency-modulated signals that approach, asymptotically, the Earth-ionosphere cavity or waveguide cutoff frequency of about 2 kHz (Fig. 1b). Both oscilloscope and spectrograph records resemble those of other authors who considered the signals were caused by lightning discharges^{2,4,8,9}. Also, visible lightning flashes were accompanied by a sharp "click" on the tape recorder monitor on other occasions.

The amplitude of the electrical noise was analysed with an electronic counter arranged to count events with amplitudes that equalled or exceeded a series of thresholds. The upper frequency limit of the counter was 200 kHz. Data for electrode spacings of 30 cm and 50 cm are shown in Fig. 2. Each point is an average of 60 s of activity. Over the range of field strengths illustrated, the slope of the line varies between -2.27 and -3.85. This curve can be used to estimate the frequency of effective noise that is perceived by an electric fish; however, some measure of threshold is needed.

Most "clicks" were brief, approximately 0.2 ms in duration, as are the return strokes of a lightning discharge¹. The behavioural threshold of an African electric fish, *Gymnarchus niloticus*, for detection of a single, 0.2 ms rectangular electrical pulse is $18 \mu\text{V cm}^{-1}$ (ref. 10). This value, indicated on Fig. 2, suggests that *Gymnarchus* should be sensitive to electromagnetic pulses occurring, on average, once every five seconds. Other threshold measurements suggest a more serious consequence of noise. *Gymnarchus* is more sensitive to pulses of longer duration, and the threshold for sensing pulse trains is far less than for single pulses. *Eigenmannia* responds to sinusoidal electrical stimuli near its own frequencies by shifting its discharge frequency away from the stimulus frequency. This response, the Jamming Avoidance Response, has a threshold sensitivity of less than $0.25 \mu\text{V cm}^{-1}$ (ref. 11). Electric fish can detect electrical noise from lightning and other atmospheric sources but the degree to which this noise interferes with communication remains unknown.

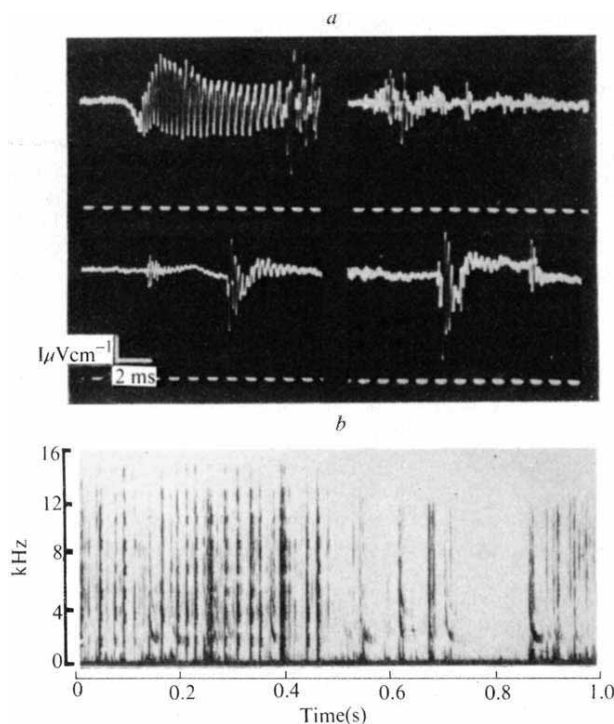


Fig. 1 Examples of naturally-occurring electrical noise recorded from Moco-moco Creek, Guyana, on June 21, 1971, at 2300. a, Oscilloscope tracings of noise at electrode spacing of 50 cm. b, Sound spectrograph of portion of recording, range 160 Hz to 16,000 Hz; bandwidth=600 Hz.

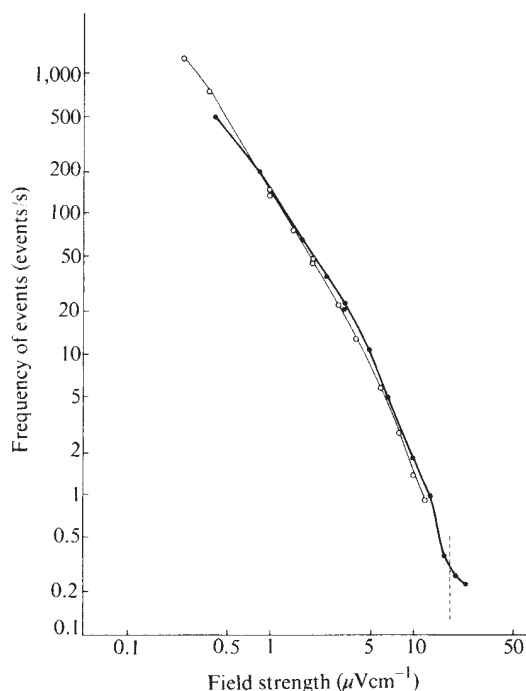


Fig. 2 Amplitude of electrical noise recorded from Moco-moco Creek, June 21, 1971, plotted against the frequency of occurrence of events of the same or greater amplitude. Electrode spacings (○—○) 30 cm and (●—●) 50 cm. The vertical broken line at $18 \mu\text{V cm}^{-1}$ is the threshold sensitivity of *Gymnarchus niloticus*.

If the social organization of electric fish requires electrical communication at a distance, then natural selection will favour communication signals that contrast with this background noise. Continuous and regular trains of pulses from gymnotid fish both with "tone" and "pulse" electrical emissions¹² contrast with the randomly-occurring lightning noise. The receiver can filter out unwanted frequencies and so increase the signal-to-noise ratio.

Certain species of electric fish such as the electric eel and some mormyrids have discharges that do not contrast with lightning noise. Their discharges, which are possibly used for object location, occur at random intervals at a low frequency. The electric eel is a predator, and to be electrically inconspicuous may be an advantage which enables it to approach prey without being detected. Small gymnotids may be part of its diet. Although mormyrids have brief, erratic discharges that must be difficult to distinguish from lightning, Moller¹³ found that *Gnathonemus niger* produced a highly regular discharge in the presence of the discharges from another electric fish; some mormyrids may therefore "regularize" their discharges, so permitting communication at a distance.

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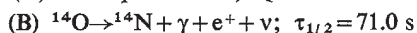
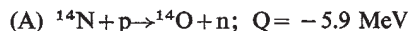
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Rapid Determination of Very Low Nitrogen Levels in Water

THE importance of nitrogen in the various life cycles of interacting ecosystems has been well described¹⁻⁴. The pertinent concentration levels may be as low as 5×10^{-9} g kg⁻¹ in water³. Quick, accurate and nondestructive tests for the presence of these levels of nitrogen are of practical importance. We report here a new approach to ¹⁴N assay, associated with the evaporated detritus of less than 4 ml. of water. The present overall accuracy is about $\pm 25\%$ at concentrations greater than 200×10^{-9} g kg⁻¹; the test is sensitive down to levels of about 20×10^{-9} g kg⁻¹. These figures can be improved by relatively simple procedures. The testing time per sample is about 3 min and the sample is not destroyed by the test.

In this method, namely charged particle (nuclear) activation analysis (CPAA), the activation (A) and subsequent decay (B) processes are



The presence of nitrogen is measured by counting the number of 2.31 MeV gamma rays emitted in reaction (B). The activation is produced by a 1 μ A beam of 8 MeV protons from a 76 cm cyclotron and the gamma rays detected with a sodium iodide scintillator following rapid transfer of the target to the counting location. After 5–10 min of decay the sample can be retested. In general, chemical tests are both highly specific and destructive; that is, they test for only one element (or even one compound of that element) and destroy or alter the sample during the test⁵. Also most chemical tests for total nitrogen detect the presence of dissolved gaseous nitrogen along with the other chemical forms of nitrogen. Our method is insensitive to volatilized nitrogen. All these differences tend to make the approaches of chemical analysis and CPAA complementary techniques.

Initial applications of the method have been made on water samples from Lake Tahoe, the Sacramento River, municipal wells, and public drinking fountains. We found that with a few simple improvements the method could be utilized to monitor a few hundred samples a day at levels greater than about 10×10^{-9} g kg⁻¹.

The method of bombardment, detection, and counting is shown schematically in Fig. 1. A 9.1 m pneumatic tube, the "rabbit tube", carried the target between the bombard and count positions with a transit time of less than 2 s. Each target was a 1.59 cm diameter tantalum foil, 0.006 mm thick with a 5 mm diameter and an indentation 1 mm deep in which the water sample was evaporated. A master timing control chassis opened and closed a shutter allowing the 8 MeV beam of protons to pass through a thin Ni foil onto the Ta target foil for a precise time. The foil was, of course, positioned in the same way from one bombardment to another. Calibration foils were interlaced with all unknown runs and the total

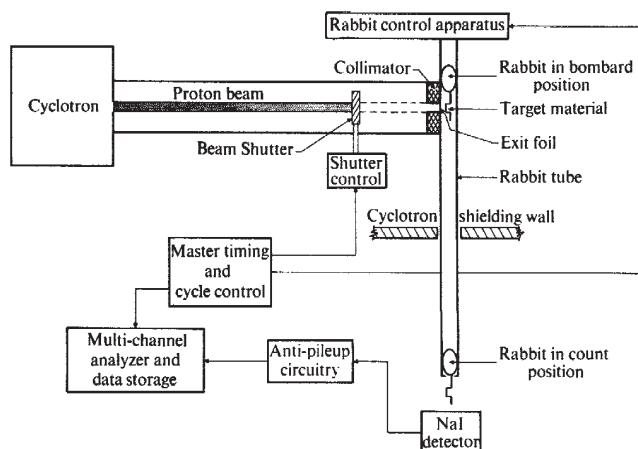


Fig. 1 Schematic diagram of bombardment and counting apparatus. The cyclotron beam was 1 μ A of 8 MeV protons. The "rabbit" is shown in both bombard and count positions in the diagram.

amount of charge was recorded and used as a relative basis of comparison. The degree of self-consistency of this calibration technique determined our ultimate accuracy ($\pm 25\%$). The overall cycle was 60 s bombard, 30 s wait, 80 s count. Further details can be found elsewhere⁶.

The detector assembly consisted of a 12.7 cm diameter by 20.3 cm long NaI(Tl) crystal mounted on a photomultiplier tube. The pulses from each event in the scintillator were stored in a multichannel analyser as counts versus pulse amplitude. Except for one sample, which showed a peak at 2.1 MeV from activation of sulphur with a 32 min half-life, the ¹⁴O decays provided the only peak in the spectrum above 1.3 MeV. The spectrum at lower energies was dominated by an intense peak at 0.511 MeV from positron annihilation following competing reactions. This potential source of electronic pile-up background was eliminated by anti-pile-up circuitry⁷ allowing detection of the desired gamma rays in the presence of counting rates up to 10^5 s⁻¹ below 1 MeV. The remaining smooth background under the peak of interest was due to electron bremsstrahlung and was easily subtracted.

Our calibration standards were prepared from water solutions with known amounts of (dissolved) AgNO₃. The calibration results are shown in Fig. 2. The scatter in the two sets of results shown is due mainly to two factors: (1) the variable amount of background nitrogen adsorbed onto the tantalum backing; for our experimental conditions this leads to a lower limit of about 20×10^{-9} g kg⁻¹ for the lowest level of detectability; (2) the clumping of the solid residue of evaporation at levels greater than 100×10^{-9} g kg⁻¹ which caused a $\pm 25\%$ scatter in the experimental results. The first problem may be ameliorated by the use of different backings, but to solve the second problem requires a somewhat more sophisticated evaporation technique than we used. Such a technique has already been used successfully for CPAA⁸ for far more heavily loaded water solutions than we used. The final result for our calibration curve is

$$10^{-9} \text{ g kg}^{-1} \text{ by weight of nitrogen} = \{ [6.1 \pm 0.2] \text{ counts } \mu\text{C}^{-1} - [32.5 \pm 8.0] \} \times 10^{-3}$$

The errors pertain strictly to the particular count cycle and sample size given above, and can of course be reduced by repeating the run an appropriate number of times but this constitutes no improvement in the method.

Table 1 gives the results of our method when applied to six different water sources, all from Northern California except for one sample from Colorado. The sensitivity of the method is also seen from the fact that the detection of approximately 20×10^{-9} g kg⁻¹ nitrogen in water requires an actual deposition on the target foil of about 80 ng of nitrogen.

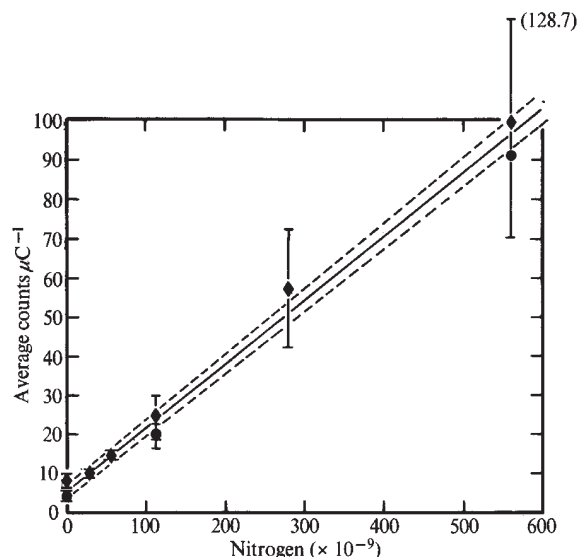


Fig. 2 Calibration curve for count/bombard cycle and beam conditions described in the text. Two sets of experimental results, shown by the two different symbols, are shown to demonstrate variability due to fluctuation in adsorbed nitrogen at low levels (less than $40 \times 10^{-9} \text{ g kg}^{-1}$) and irregularities in sample preparation at high levels (greater than $100 \times 10^{-9} \text{ g kg}^{-1}$). —, Plot derived statistically using all the data; ---, present error limits.

It is not surprising to find that the Lake Tahoe sample has the lowest value of nitrogen contamination in the table, as it is a good example of an oligotrophic lake³, that is, very low in organic growth. Samples 2 and 3 (Table 1) are from different sources at the University of California, Davis, where the water distribution system has not undergone any major change for the past few years. Accordingly it is felt that the change in nitrogen level in sample 3 for the two dates, one year apart, is very probably due to a change in the well-sources feeding the university system. This is supported by sample 4, where we see the widely divergent results of three different well sources from the city of Davis water supply system. Well No. 18 is a new, 1972, well, and an independent chemical test (D. Pelz, Public Works Director, Davis, private communication) for NO_3 (nitrate) in the water from this well and Well No. 10 gave results of $3,160$ and $925 \times 10^{-9} \text{ g kg}^{-1}$ respectively (error about $\pm 10\%$ in both cases) of nitrogen in this form. As these figures agree reasonably well with our results for total nitrogen we suggest that most or all of the nitrogen is in the NO_3 form in both wells. Neither figure approaches the present US Public Health Service limit of acceptability, which is $10,000 \times 10^{-9} \text{ g kg}^{-1}$. Another conclusion from the comparison of our results and those of the chemical analysis is that the loss of nitrogen during evaporation is not likely to originate from the nitrate form.

Our results represent only preliminary indications of the

Table 1 Application of Nitrogen-Activation Method to Various Water Sources

Sample origin	Date	$\text{g kg}^{-1} \times 10^{-9}$
1. Lake Tahoe, California; supplied Dept. of Zoology, Univ. of Calif., Davis	March 1972	21 ± 16
2. Tapwater, Environmental Eng. Lab., Univ. of Calif., Davis	14/6/71	45 ± 31
3. Drinking water, Walker Hall, Univ. of Calif., Davis	14/6/71 4/ 7/72	80 ± 42 340 ± 110
4. City of Davis, California		
Well number 10	7/ 6/72	755 ± 35
Well number 17	7/ 6/72	180 ± 45
Well number 18	7/ 6/72	$2,835 \pm 266$
5. Sacramento River, Three-Mile Slough	April 1972	83 ± 43
6. Colorado River, below Lake Havasu	23/3/71	400 ± 80

potential of our approach to the problem of low-level nitrogen monitoring. Two important but simple improvements have already been mentioned, and with these the accuracy at higher concentrations would be essentially unlimited (except for the pile-up effects already mentioned) and the lower limit of sensitivity would be about halved. The relatively modest accelerator requirement, that is, an 8 MeV proton beam of about $1 \mu\text{A}$, makes the method universally applicable.

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Odour-Blindness to Musk: Simple Recessive Inheritance

THREE types of odour-blindness or specific anosmia have been studied, but genetical evidence so far obtained is tentative or inconsistent¹. The rare anosmia to the *n*-butylmercaptan of skunk², and more commonly the scent of freesia flowers³, may be inherited as autosomal recessive traits. Anosmia to hydrogen cyanide⁴⁻⁶ is complex and its inheritance has not been confirmed⁷.

Specific anosmia to the musk pentadecalactone⁸ (Thibetolide[®], Givaudan Corp., New Jersey) was found in about 7% of tested subjects. The individual odour detection thresholds for ninety subjects tested with a dilution series are shown in Fig. 1. The saturated solution (dilution 0) 1.1 p.p.m., w/v, was prepared by shaking 1.1 mg of pentadecalactone in 1 l. of water after warming to 40° C to melt the musk and binary dilutions made from this. The test procedure and necessary precautions have been described^{9,10}. The majority of subjects (eighty-four) had thresholds forming a roughly Gaussian curve between dilutions six and fourteen, having a mean normal detection threshold of 10.1 dilution steps (exactly 1 part of musk in 10⁹ of water), and standard deviation ± 1.48 steps. A minority of six subjects (6.7%) had thresholds at higher concentrations, and one could not smell the saturated solution. To make an arbitrary distinction between normal observers and specific anosmics, we used step 6.5 on Fig. 1 (0.0123 p.p.m. pentadecalactone in water) which represents twice the average standard deviation for 18 other odorants⁹.

Anosmia to the sweaty odour of isovaleric acid affects about 2% of the population¹¹. The corresponding diagnostic strength for purified⁹ isovaleric acid is 2.35 p.p.m. in 0.01 N H_2SO_4 . As a control we used 41.1 p.p.m. purified isobutyl isobutyrate. This complex fruity odour screened out any general anosmics having little or no sense of smell (about 0.2% of the population⁹).

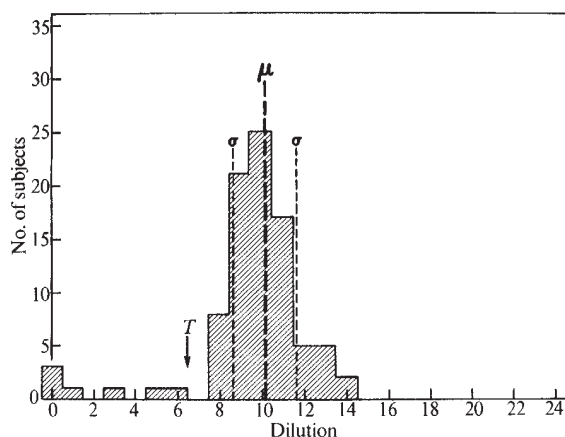


Fig. 1 Olfactory thresholds of ninety persons to pentadecalactone dilutions in water. μ , Mean normal threshold; σ , standard deviation; T , test concentration for distinguishing specific anosmics.

Our modified test procedure was conducted in an odour-free environment. The subject received a tray with 3 glass-stoppered 125 ml. Erlenmeyer flasks, one containing 20 ml. of odorous solution and the other two odourless water in a double-blind trial. The trays were presented to the subjects in the sequence: isobutyl isobutyrate, isovaleric acid, pentadecalactone, and the flask with odour selected by sniffing. When a decision was recorded for each tray, the flasks are randomized again and the series repeated. A subject was scored as a non-smeller of the test compound if he failed on either trial. Odorous flasks were used for not more than 10 subjects before replacing the solution. The subjects did not wear perfume, and refrained from eating, drinking or smoking for 15 min before the test, as these factors temporarily decrease olfactory sensitivity⁹. Any kind of nasal pathology such as colds, allergies or sinusitis may cause a loss of sensitivity¹² averaging 2 dilution steps⁹.

We present results on 109 Caucasian families in whom all known inherent and environmental interferences with olfactory sensitivity have been excluded. Although adult olfactory acuity decreases somewhat with age¹³, there was no significant difference in the incidence of anosmia to isovaleric acid or pentadecalactone between the 218 parents (average age 41.2) and 266 children (5 to 18 years, average 12.1). None of the 484 subjects failed to smell isobutyl isobutyrate, 6 (1.2%) failed isovaleric acid, and 42 (8.7%) failed musk, frequencies agreeing with those reported earlier^{8,11} and shown in Fig. 1.

The isovaleric acid data implied recessive inheritance but were too meagre for analysis. Insensitivity to musk occurred in thirty-six families (Fig. 2); males and females were equally affected. The offspring from twenty marriages between smellers were analysed by the *a priori* method¹⁴ (Table 1), and there was almost exact correspondence between the expected and observed results.

Among the fifteen marriages between smeller and non-smeller, thirteen produced only smeller children, probably because of small family size. Two marriages produced a total of three children, one smeller and two non-smellers. A marriage between non-smellers produced two children, both non-smellers.

The data acquired with this test protocol, though not conclusively ruling out polygenic inheritance^{14,15}, strongly suggest that the inability to smell pentadecalactone is inherited as a simple recessive autosomal character. The high incidence of the anosmia suggests a genetic polymorphism, but might result from relaxation of natural selection on the human olfactory sense.

There was no evidence of non-random mating or selection for this trait, so that the observed recessive proportions from

Table 1 Segregation Analysis of the 20 Families with Normal Parents and at Least One Affected Child (*a priori* Method)

Size of sibship (n)	No. of sibships (x)	q'n*	Total affected	
			Expected q'nx	Observed
1	2	1.000	2.000	2
2	5	1.143	5.715	7
3	8	1.297	10.376	11
4	1	1.463	1.463	1
5	2	1.640	3.280	2
6	2	1.825	3.650	2
20†			26.484	25

$$* q' = \frac{1/4}{1 - (3/4)^n}$$

† Total: 62 children.

the various marriage types should fit those expected for a population in equilibrium with $q=0.2945$. This was not true. Scrutiny of the data revealed two groups distinguishable by national origin. In fifty-five families the parents were at least third generation Americans of Western European descent. In fifty-four families the parents had migrated after the Second World War from Western Europe or had immigrated from outside Western Europe. In the first group ($q=0.2577$) gene frequency analysis showed agreement between observed and expected results, but in the second group ($q=0.3270$) it did not. We attribute these results to the different degrees of national homogeneity discernible in the family histories.

We have found significant differences in the frequencies of specific anosmias between two human races. Isovaleric acid anosmia is prevalent (9.1%) among our sample of seventy-seven Negroes and uncommon (1.4%) in our sample

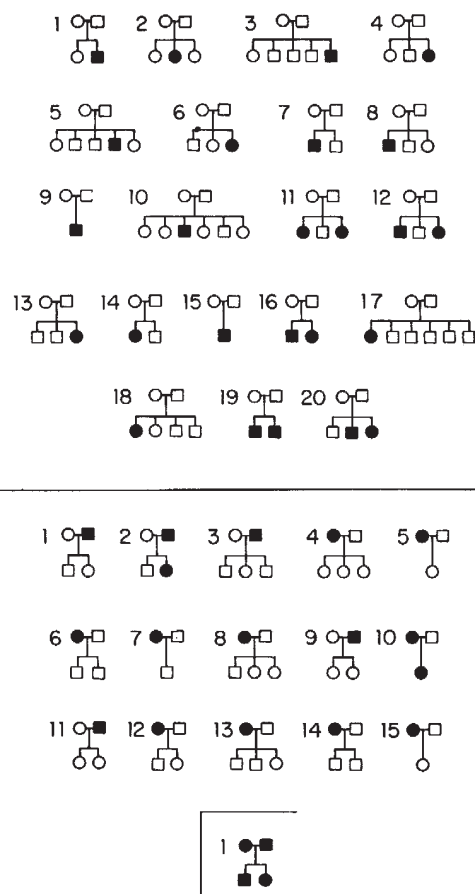


Fig. 2 Family trees showing the occurrence of odour-blindness to pentadecalactone (solid symbols).

of 849 Caucasians. Musk anosmia occurred in 7.2% of the Caucasians but in none of the Negroes. We suspect wide variations in the incidence of specific anosmias within races.

The origin for specific anosmia could be a defect in the hypothetical olfactory receptor protein, representing one of the physiological primary odours (such as sweaty, musky)^{10,16}. An essential corollary of the hypothesis is the demonstrated simple recessive inheritance of specific anosmia, which is only expected if the test odorant is very close to a pure primary odour.

The chemical mapping of specific anosmias is the most logical and practical approach to classifying the primary odours in the human sense of smell^{10,11,17}. We think that the study of genetics of the twenty-seven known discrete anosmias⁸ would provide many new and accessible polymorphisms for the study of human genetic variation¹⁵.

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Chlordimeform: a Pesticide Acting upon Amine Regulatory Mechanisms

CHLORDIMEFORM (also known as chlorphenamide or Galecron) is a relatively new acaricide-insecticide of which the mode of action has not been elucidated. It is an economically important pesticide^{1,2} and is also an interesting compound from a toxicological point of view, as it is effective against many organophosphate and carbamate resistant pests³, and causes distinctly different symptoms in insects from conventional insecticides. In addition, it is of pharmacological interest because it belongs to a novel group of insecticides which possess a formamidine moiety.

We have observed that chlordimeform at a high dose (200 mg kg⁻¹ injected intraperitoneally (i.p.) approximately LD₅₀) causes marked hyperexcitation in a short time period (5 to 10 min) in rats and mice. They exhibit tremors and become extremely hypersensitive to external stimuli. The poisoned

Table 1 MAO Inhibition * by Chlordimeform Analogues and by Known MAO Inhibitors

Structure	pI_{50} *	Name
	5.46 ± 0.14	Phenelzine
	3.50 ± 0.08	Iproniazid
	4.49 ± 0.05	Chlordimeform
	4.60 ± 0.05	C-8520
	3.85 ± 0.03	C-4789
	3.81 ± 0.07	C-4789
	3.23 ± 0.01	C-4789

* Data expressed as $pI_{50} \pm$ standard deviation where $pI_{50} = -\log I_{50}$, I_{50} = inhibitor concentration in mol l⁻¹ (final concentration) giving 50% inhibition. Each value is mean of 3–5 determinations. Inhibitors were added to reaction tubes in 20 μ l. solvent (H₂O or 95% ethanol). Control tubes received solvent alone. Preincubation was begun by addition of enzyme (0.1 ml. of filtered liver homogenate). The reaction was initiated by addition of 0.3 μ mol of the substrate kynuramine dihydrobromide in 20 μ l. of H₂O.

animals show signs of locomotive difficulties partly due to frequent hyperextension of hind legs at later stages.

In sublethal doses (50–200 mg kg⁻¹ i.p.) chlordimeform has a sedative effect. The poisoned animals, when placed in an open area, stay motionless in a characteristic low posture unless they are externally stimulated. In rats at all doses tested (50–200 mg kg⁻¹) gradual dilation of pupils took place over a 1 h period. In no cases did the animals show such cholinomimetic symptoms as slowing of heart beat, salivation, urination, or fasciculation, which agrees with the *in vitro* observation that chlordimeform is not an inhibitor of housefly head cholinesterase. Rather, the overall symptoms of chlordimeform poisoning are sympathomimetic in nature.

To study the biochemical cause of chlordimeform poisoning, we have examined its effect upon the monoamine oxidase⁴ of the rat liver. Livers were homogenized in 5 volumes of cold distilled water. The homogenate was filtered through glass wool and used directly as the enzyme source. Enzyme and inhibitors were preincubated for 15 min at room temperature. Enzyme activity was defined as the amount of kynuramine metabolized in 20 min at 37° C.

The results (Table 1) clearly indicate that chlordimeform is an inhibitor of monoamine oxidase (MAO). Also, the degrees of inhibitory potency of the chlordimeform analogues are roughly correlated with those of the general *in vivo* toxicity of these compounds⁵ to mites. It is expected that some biogenic amines accumulate as a result of monoamine oxidase inhibition *in vivo*. To examine this possibility male rats were treated with 200 mg kg⁻¹ (intraperitoneal injection) of chlordimeform. The animals were killed after 1 h and the serotonin and norepinephrine levels in the whole brain were determined by the method of Maickel *et al.*⁶. It was found (Table 2) that the levels of serotonin and norepinephrine in the treated animals were 70% and 22% higher than in control animals.

It is premature to conclude that this inhibitory property of chlordimeform is the sole mechanism by which it poisons the animal. At least some of the symptoms observed during chlordimeform poisoning, however, can be attributed to the

Table 2 Serotonin and Norepinephrine Levels* in Whole Rat Brain

	Chlordimeform	Control
Serotonin	0.75 ± 0.07	0.44 ± 0.06
Norepinephrine	0.22 ± 0.01	0.18 ± 0.03

* Data expressed as $\mu\text{g g}^{-1}$ wet weight ± standard deviation. Averages of six animals each for serotonin and three animals each for norepinephrine. Data uncorrected for extraction efficiencies. Male 150 g rats were used. Animals were injected intraperitoneally with 200 mg kg^{-1} of chlordimeform (hydrochloride salt) in aqueous solution, injection volume = 0.7 ml. Control rats received 0.7 ml. H_2O . Animals were sacrificed 1 h after injection.

accumulation of amines. The overall description of serotonin poisoning⁷ in the presence of a monoamine oxidase inhibitor closely resembles that for chlordimeform, and chlordimeform (50 mg kg^{-1} rat, i.p.) antagonizes certain of the symptoms of reserpine intoxication (such as tremors and closed eyelids, with a reserpine dose of 10 mg kg^{-1}).

Another possibility is that chlordimeform can directly react with the receptors for the amines. As these two reactions (MAO inhibition and direct action) could elicit similar poisoning symptoms, we cannot eliminate the possibility at this stage that the latter mechanism could also play an important role in the poisoning process.

Our finding that an insecticidal compound can interfere with amine regulation in animals is entirely new. It is significant in view of the preliminary evidence that chlordimeform does not interact with any other well established biochemical target systems that are known to be related to insecticidal action. A new class of insecticide, acaricides, which act on a different pharmacological principle is important in the light of the urgent need to develop good substitutes for environmentally hazardous pesticides.

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Use of Male Sterilization Mutations for Insect Control Programmes

RESISTANCE and deleterious environmental side-effects have limited the use of insecticides in insect control programmes, and researchers have become increasingly aware of the potential of population control by genetic manipulation. In pioneering experiments, screw-worm fly eradication was accomplished by the release of sterile males into natural populations, and wide use of this method was anticipated¹. Serious drawbacks, however, have often limited the successful utilization of this approach². Males are usually sterilized by the induction of dominant lethal mutations in a high proportion of sperm and

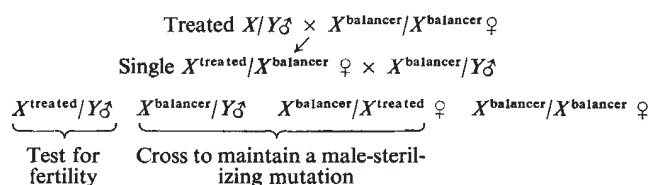


Fig. 1 A scheme to isolate sex-linked male-sterilizing mutations. $X^{balancer}$ indicates an X -chromosome that suppresses recombination and bears a mutation with a visible phenotype.

spermatids by treatment with ionizing radiation or chemical mutagens. In some species the treated males do not compete successfully with males from the natural population for mating partners; in others the sterile males may not trigger a monogamous response in mated females, or male fertility is regained with time.

Release of insects which will have a detrimental impact on natural populations based on other mechanisms has been suggested, including cytoplasmic incompatibility, hybrid sterility, meiotic drive, distorted sex ratios, and conditional lethal factors³. Release of translocation- and compound chromosome-bearing insects has been advocated, and there are programmes to recover and characterize translocations for a number of species^{4,5}.

I suggest that the release of males bearing sterilizing mutations could avoid some of the current drawbacks of induced male sterilization for some insect species. In species with an X/Y system of sex determination, such mutations are easily generated by recovering mutagenized X -chromosomes in females and screening their sons for fertility (see Fig. 1). The mutagenized X -chromosome is recovered heterozygous with an X (denoted "balancer") which has one or more inversions to inhibit recombination and a mutation with a visible phenotype.

I collaborated with Drs D. L. Lindsley and E. Lifschytz in an experiment to generate sex-linked male-sterilizing mutations in *Drosophila melanogaster* by this method. Males were treated with ethyl methanesulphonate at a dose which produced sex-linked lethal mutations on 36.5% of the chromosomes tested. Among the lethal-free X -chromosomes, 4.7% carried an induced male-sterilizing mutation. The nature of the sterile syndrome has been preliminarily characterized for 192 independently generated mutations, and a large proportion are associated with aberrancies of spermatogenesis such that mature, motile sperm are not produced. Twelve mutations which allow males to produce motile sperm have been more extensively characterized (R. E. Denell and M. C. Lim, unpublished results). Males bearing two of these mutations do not copulate; males bearing four mutations have low numbers of weakly motile sperm in the seminal receptacle, but their ejaculate is aspermic; males hemizygous for three other mutations transfer sperm to females, but the sperm are incapable of reaching the female sperm storage organs. Finally, three mutations allow males to produce apparently fully motile sperm which are transferred to females, stored, and disappear from the storage organs at about the same rate as normal sperm. Examination of the eggs laid by such females shows that sperm have not entered them, and an abnormality of the sperm acrosome is implied.

Male-sterilizing mutations have also been induced in *Drosophila* by ionizing radiation, and isolated from natural populations⁶. If *Drosophila* is regarded as a model insect system, male-sterilizing mutations should be easily generated in other species. Eliminating the relatively rare class of mutation which affects mating ability directly, sterile males should be as sexually competitive as any laboratory raised strain. (It would be best to use low mutagen doses to minimize the induction of linked detrimental mutations, although detrimental could be removed by recombination.) Sterility is immediate and (with the exception noted below) permanent. A preliminary characterization of mutations should allow the choice of one which causes monogamous females to reject further suitors. For

polygamous insects, the choice of sterile males with actively motile sperm should allow these gametes to compete effectively with normal sperm for storage and fertilization.

In the experiment described above, fertility was initially assayed at 27° C. Later retests of males developing at 19° C showed that about one-third of these mutations are temperature-sensitive, and males are fertile. Smith⁷ has discussed the release of insects bearing temperature-sensitive lethal mutations for control programmes. If a useful temperature-sensitive male-sterilizing mutation is isolated, a homozygous stock could be expanded to large numbers at a temperature allowing fertility. Culturing an additional generation at a sterilizing temperature and separation of the sexes would provide sterile males for release. Care must be taken, however, that males bearing the mutation will not regain fertility under temperature conditions occurring in the wild. Also, some stocks of temperature-sensitive male-sterilizing mutations appear to accumulate genetic modifiers with time, so that males become fertile at temperatures which formerly sterilized them. Thus any mutant being considered for release must be carefully characterized.

A conventional male-sterilizing mutation could also be utilized for release. In the scheme in Fig. 1, a balancer *X*-chromosome is used. A balancer could be induced by ionizing radiation or chemical mutagens which cause chromosomal rearrangements, and could be screened by testing for crossover suppression of sex-linked genes, and/or by polytene or meiotic chromosome analysis if they are possible. A balancer is crucial for the rearing of large numbers of sterile males. This would be greatly aided by the induction of a female-sterilizing mutation on the balancer; such mutations are also easily induced in *Drosophila*⁶. Thus a "balanced stock" can be established by crossing $X^{\text{balancer}} \text{♀-sterile}/Y$ males to $X^{\text{balancer}} \text{♀-sterile}/X^{\text{balancer}} \text{♀-sterile}$ females. Among the progeny of this cross, only the parental genotypes are fertile, and (with appropriate monitoring for rare events that cause the system to break down) the stock could be expanded to produce vast numbers of sterile males.

A further sophistication would be the induction of some mutation to facilitate mass isolation of sterile males. Such a mutation should be carried by the balancer chromosome so that sterile males remain maximally vigorous, and should be dominant. Temperature-sensitive dominant lethal mutations or behavioural mutations affecting geotactic or phototactic response or causing paralysis should be useful in this context.

My discussion has dealt with the induction of *X*-linked sterilizing mutations in species with a heterochromatic *Y*-chromosome, but the basic concepts are easily applied to species with other sex chromosome systems or for autosomal male-sterilizing mutations, and only small procedural changes are necessary.

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Intra-population Differentiation of Physiological Response to Flooding in a Population of *Veronica peregrina* L.

WHEN a plant population grows in a patchy environment, it can be exposed to a variety of selective pressures. If the resulting disruptive selection is strong enough, intra-population differentiation can occur over distances of a few metres, in spite of the absence of any barriers to gene flow^{1,2}. We wish to report an example of such differentiation within a population of the annual forb *Veronica peregrina* L.: the differentiation is with respect to physiological responses to varying régimes of soil moisture.

V. peregrina is apparently a native of the Western Hemisphere, including California, and grows in moist to wet habitats^{3,4}. The population studied grows within and around the periphery of a temporary pool in Solano County, California. The pool is about 35 cm deep at its deepest spot and measures about 4 by 10 m. It is filled by seasonal rains which fall approximately from December to March and trigger the onset of germination of *V. peregrina* seed. Seed in the deepest part of the pool, near the centre, germinate under water and give rise to individuals which can be under water or in water-logged soils for 2 months or more. Conversely, individuals arising from seed at the periphery of this pool are seldom under water or in water-logged soil except for short periods after heavy rains. Thus different portions of the population are exposed to very different regimes of soil saturation.

When plants grow in saturated soil, their roots are in oxygen-limiting or anaerobic conditions. Previous work suggests that plant species will have very different physiological responses to such conditions, depending on whether they are, by nature, intolerant or tolerant of flooding⁵⁻⁷. In species intolerant of flooding, the oxygen-limiting conditions cause an acceleration of glycolysis and a subsequent accumulation of acetaldehyde and ethanol which have injurious effects on the plants. Furthermore, a "malic" enzyme decarboxylates the malate present to pyruvate, contributing further to ethanol production. In tolerant species, there is no acceleration of glycolysis, "malic" enzyme is not found, and non-toxic malate is the end product of anaerobic respiration⁷. Our principal aim was to see whether such differences in malate accumulation could be found within a population by comparing individuals from the centre with others from the periphery. It should be stressed that the opportunity for genetic exchange by means of seed and pollen between centre and periphery is apparently present because the population is continuous and seed is scattered from the seed capsules which burst open at maturity. Furthermore, though this species is predominantly self-pollinated, some cross-pollination may occur because the flowers are protogynous⁸ and have some insect visitors^{8,9}.

Ten families originating from the centre were compared with ten families originating from the periphery. In all cases, a family was made up of individuals grown from seed collected from one mature plant in the field. Two sets of twenty-five seeds per family were germinated on wet soil with one set of one family per pot. The plants were allowed to grow for about 30 days at which time they were 2-5 cm tall and had roots reaching the bottom of the 9 cm-high pots. Then one set of all twenty families was placed in deep metal trays which were filled with water. The water level was kept about 1 cm above soil level at all times, so that the soil was saturated. The second set was kept in flats and was watered daily but excess water drained off freely. Both sets of families were maintained at the experimental regimes for 4 weeks, and then assayed for malate contents. To run the assay, roots were collected and thoroughly washed in running water to rid them of any pieces of extraneous organic material. For each family, the roots were prepared according to the method outlined by Bergmeyer¹⁰. The malic acid contents were assayed using a modification of the enzymatic method outlined by Bergmeyer¹⁰.

To determine the amount of malic acid in each experimental tube, these tubes were compared visually to a set of standards prepared by serial dilution and containing known quantities of malate. All experimental tubes and standards contained equal volumes of solution. We were primarily interested in a precise comparison of performances of families from the centre versus those from the periphery. Consequently, pairs of families, one from the centre and one from the periphery, were kept adjacent to one another throughout the experiment. The results were analysed using the Wilcoxon signed-ranks test which is most appropriate for paired comparisons.

Table 1 Malate Contents ($\mu\text{g/ml}$ of Root Extract) of Roots of Centre Families (C) and Periphery Families (P) Grown in Moist and Wet Soil

Pair	Soil conditions				Reaction to increased moisture*	
	Moist	P	Wet	P	C	P
1	0.40	0.65	1.20	0.15	+	—
2	0.40	0.65	9.60	0.15	+	—
3	0.40	0.65	4.80	2.40	+	+
4	0.40	0.65	2.40	1.20	+	+
5	0.40	0.65	1.20	9.60	+	+
6	0.40	0.65	1.20	1.20	+	+
7	0.40	0.65	2.40	0.60	+	—
8	0.65	0.40	1.20	0.60	+	+
9	0.40	<0.01	4.80	0.30	+	+
10	0.05	0.40	13.00	0.15	+	—
$\bar{X}$	0.39	0.53	4.18	1.62	+	+
Stat. signif.	NS		$P < 0.025$		$P < 0.001$	

* This column represents the change in malate contents as soil moisture is increased.

Table 1 shows the malate contents in roots of all families in moist and wet conditions. In moist conditions, roots of families from the periphery contain slightly more malate than roots of families from the centre, but the difference is not statistically significant. In family pairs grown in saturated soil, however, there is a much greater accumulation of malate in the roots of centre families. This difference is statistically significant. A summary of the reaction to an increase in soil moisture and concomitant decrease in soil oxygen is provided in the third column of Table 1. In all ten families from the centre, there was a sizable increase of malate accumulation. The response is statistically highly significant. In periphery families, there was an increase in six families and a decrease in four families. The average response was an increase which is not statistically significant.

The significant difference in the response to flooding of centre and periphery plants can be understood in terms of the difference in habitats occupied by those plants. Four families from the periphery where soils are seldom saturated showed a decrease in malate contents which may be due to the triggering of "malic" enzyme activity⁷. Six families from the periphery showed some increase in malate contents suggesting they are at least somewhat adapted to flooded conditions. In all families from the centre where flooding is commonplace, the response was a significant increase in malate contents and this increase was significantly larger than the increase in periphery families. Intra-population differentiation for several other characteristics has also been observed within the *V. peregrina* population studied⁹. The distance between the centre and periphery of this population is 2–5 m depending on the location, and the observations reported suggest that differentiation, perhaps produced by disruptive selection, can be maintained over such short distances.

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Pollution of Beaches: Reply to Regnier and Park

Regnier and Park¹ do not appear fully to have appreciated the significant point that emerged from my finding² that much higher concentrations of antibiotic-resistant and sensitive coliform organisms were present in specimens of water taken from rivers flowing through urban areas than were present in similar specimens taken from rivers flowing through rural areas. This is that human beings were the main source of the sewage pollution of the rivers and that domestic animals were a comparatively unimportant source. The fact that one cannot differentiate the two kinds of specimens from each other by reference to the ratio of the numbers of resistant and sensitive organisms they contain is therefore irrelevant.

Because of my observations on river waters, domestic animal sewage was not thought to be worthy of consideration as a major source of the pollution I found during my later observations on coastal bathing waters³. Also, there have been several reports, including the one referred to in my paper⁴, implicating human sewage in this respect and, as one would expect, there was no evidence of any farm waste being drained into the beaches I examined.

My comment that the finding of R⁺ *Escherichia coli* in sea water provides "stronger evidence of contamination with human sewage", to which Regnier and Park specifically refer, was not made in reference to the possible implication of domestic animal sewage but in reference to the possible implication of excreta from marine life and wild birds—this is clearly stated in my paper.

In view of the many reports of European beaches being closed because of human sewage pollution and even of people being fined for bathing in them, I am surprised that Regnier and Park adopt what, to me, seems a rather complacent attitude to the state of our beaches. I cannot share their view. Although I would not class most of the ones I examined as grossly polluted, some of them, Penarth and Ogmore for example, must have been getting near to qualifying for that definition.

Finally, with regard to the public health aspect, I feel that hygienic principles can mean very little if they condone the immersion of the human body in what are really suspensions of human faeces.

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- ¹ Regnier, A. P., and Park, R. W. A., *Nature*, **239**, 408 (1972).
- ² Smith, H. W., *Nature*, **228**, 1286 (1970).
- ³ Smith, H. W., *Nature*, **234**, 155 (1971).
- ⁴ Report, *J. Hyg. Camb.*, **57**, 435 (1959).

BOOK REVIEWS

Ecology of the Waters

Marine Ecology: a Comprehensive, Integrated Treatise on Life in Oceans and Coastal Waters. Edited by Otto Kinne. Vol. 1. *Environmental Factors*, Part 3. Pp. ix+1245-1774. (John Wiley: New York and London, February 1972.) £12. *The Estuarine Environment.* Edited by R. S. K. Barnes and J. Green. Pp. xii+133. (Applied Science: London, 1972.) £3.50.

Coastal Zone Management: Multiple Use with Conservation. Edited by J. F. Peel Brahtz. Pp. xii+352. (John Wiley: New York and London, March 1972.) £7.65.

IN 1972, a year marked by the centenary of the Challenger Expedition and a vocal concern for the state of the global environment, it seemed appropriate to consider three books which could contribute to a more rational management of a part of that environment. The first two contain information of a kind essential to those individuals empowered to undertake sound management of coastal areas and who thus may use the third as a manual of strategy. Each is composed of review papers and as such has a considerable potential value for the prospective audience. However, it is clearly not easy to ensure that the constituent papers have a uniform quality, and unfortunately these works do not escape this fate. Additionally, such works are evidently exposed to the danger of oversimplification and are peculiarly vulnerable to a critical quotation of counter examples; wisdom would therefore suggest that exaggerated claims regarding objectives and scope should be avoided. Because such books cover a wide field, much of it beyond the scope of expertise of the individual, the books' reviewer must judge the whole upon those sections in which he has some knowledge. Each of us must surely be well aware of the ease with which the vital reference is missed and of the appalling deficiencies of one's own knowledge; therefore carping criticism is an idle exercise.

Marine Ecology, volume 1, part 3, which is a part only of an ambitious, five volume, multi-part work, has the misfortune to suffer all the faults noted.

The publisher informs the prospective reader, drawn hopefully from a professional and lay audience, that it is intended "as an exhaustive systematic exposition summarizing and evaluating information obtained thus far on living systems in the sea and littoral areas". In six chapters, it deals with substratum, pressure, dissolved gases, organic substances, ionizing radiation and factor combination. Although one must be impressed both by the laudable and worthwhile objectives expressed and by the useful contribution which this work will make, one is left with an overwhelming sense of disappointment in its execution.

From internal evidence, one concludes that the information explosion has obliged the publisher to limit the number of references quoted, but why with respect to substratum, for example, are so few references quoted for the period since 1965? Why too are illuminating contributions ignored? As an example among several, is the work of D. P. Wilson who has contributed so much to the understanding of the mechanism of settlement by littoral invertebrates really not worthy of inclusion here?

The inconsistency between pages 1250 and 1257 regarding the ecological importance of the mineralogical composition of sediments is worrying, but should have been resolved during editing. Much more disturbing are the faults typified by that on page 1274 relating to the migration of littoral diatoms. Contrary to the statement contained therein, the ability to undertake tidal or diurnal migration is not confined to the naviculoid diatoms, nor are the *Hantzschia* spp. particularly outstanding in this respect. Indeed, a wide literature gives a very different view of this aspect of unicellular plant behaviour, which is exhibited by *Euglena* sp. and dinoflagellates in addition to the diatoms.

Regretfully, one must conclude that this is a useful, though not completely accurate, introductory work which fails to match the publisher's claims. This is a very great pity, for there is an undoubted need for an exhaustive work based on this conception and one hopes that in future parts such pitfalls will be

avoided and the ambitions of all concerned with them realized.

The Estuarine Environment consists of eight papers presented at the symposium held to mark the inauguration of the Estuarine and Brackish-water Sciences Association. The papers thus offered are concerned to present an up to date introduction to some aspects of the physics, chemistry and biology of estuaries; this they do with particular reference to those of Britain. The more outstanding papers are those of K. R. Dyer, R. L. Jefferies and P. R. Walne concerned with sedimentation, holophyte nutrition and the importance of estuaries to commercial fisheries respectively. With a multidisciplinary audience in mind one would have hoped, for the benefit of the non-botanist, that some clarification of the status of *Spartina*, other than the bald reference to it as *S. anglica*, would have been made.

The paper by H. Milne and G. M. Dunnet is concerned with the productivity relationships of the Ythan estuary, Aberdeenshire. They and co-workers have quantified the trophic ecology of some fish, viz., goby and flounder, and birds, viz., oystercatchers, greater black back and herring gulls and eider; although other trophic relationships have not been quantified, food webs for the mud-flat and mussel bed communities are presented. Apparently, *Carcinus* and *Littorina* resident in the Ythan differ, in their trophic ecology, from populations resident elsewhere; an elaboration of results with respect to these species would have been a most valuable addition here. Hopefully, this work will be published elsewhere shortly.

In general this is a book which does much of what it sets out to do; it is, however, an expensive purchase at £3.50; indeed, at this price the reader is surely entitled to a more uniform presentation of the figures than the uneven quality provided.

The eleven papers of *Coastal Zone Management* are concerned to provide a unified approach which embraces politics, conservation, social needs, problems concerned with shipping and ocean installations, and marine waste disposal systems. This is a valuable

work which demonstrates clearly both the conflicting interests and the intrinsic problems facing those who would see coastal areas managed rationally. It is unashamedly devoted to the problems of the USA. As such, knowledge from other sources is not used fully and thus its value to the cosmopolitan audience may be reduced. Truly outstanding is the exposition by E. A. Pearson of the issues which face the would-be manager of effluent releases. Too often, treatment and the pipeline have been regarded as mutually exclusive solutions to the waste disposal problem; that this is not so is adequately demonstrated in this clear account. E. J. PERKINS

Matter and Life

Molecules to Man. By Sir George Porter, Richard J. Harrison, Paul R. Ehrlich, D. C. Phillips, G. V. J. Nossal and Chapman Pincher. Pp. 410. (Heinemann: London, December 1972.) £2.90.

SOME thirty years ago a series of talks introduced by Sir William Bragg were published by the BBC. Their intention was to show the continuity of principle between studies of atoms and molecules and the behaviour of genes, cells and organisms. The same idea seems to underlie the series of lectures at the University of Sydney which are collected in this book.

It certainly underlies the introduction by Bragg's successor, Sir George Porter, with his elegant account of theories of matter and of life. These connect very neatly with the nucleic acids and proteins which Professor Phillips uses to authenticate, as it were, the mechanism of heredity. Perhaps genetic principles are taken too much for granted at this point, for at the next step we find Professor Nossal grappling with immunity, a problem the crux of which is variation as much as heredity. Indeed, Burnet's theory of clonal selection among variant cells may be seen as assuming three levels of genetic control in variation. But it leads us to ask which of these levels are in the nucleus and which in the cytoplasm, which operate through DNA and which through RNA.

Nossal passes on to connect the immunity system and the study of cancer. He separates cancer induced by chemical carcinogens, cancer propagated by viruses (or proviruses), and cancer as an embryonic property. In immunity reactions these classes may differ but need they be mutually exclusive? Nucleus and cytoplasm as places of origin must be mutually exclusive. This separation is the inescapable one.

From immunity it is a long jump to the environment. Here Professor

Ehrlich is concise and effective. His arguments are by now well known. But behind the visible issues are conflicts of vested interest which need to be better known. They are of two kinds. One is between rich people or advanced countries who produce (and consume) too many goods and poor people or backward countries who produce too many children. The other conflict is between people who are accused of exploiting the physical sciences to the damage of the environment, and people who, mainly on biological evidence, make this accusation.

To recognize these conflicts is to take one step towards resolving them. A second is to recognize that beneath them are instincts for competition between people who firmly believe that they differ in intelligence. Such properties, whether of competitiveness or intelligence, have dominated man's past and, since they are inherited, they are not likely to be dismantled by exhortation. They therefore threaten us with uncontrollable dangers as the world's population approaches the limit which Ehrlich foresees.

To conclude, Mr Chapman Pincher gives his candid views on what the public should be told about science. He reveals the dilemma of the communicator who has to believe in the education of the masses (our masters, he says) although their demand for horoscopes makes him doubt the good it has done them. What they want is stories of sex reversal, instances of sex violence, and perhaps arguments for sex equality. Some of these Mr Pincher is able to provide.

At this point the reader may turn back to the middle of the book. Here there might have been some biological evidence which would have helped the discussion of sex in a serious way. Instead a hundred pages have been inserted on porpoises, pages unconnected with the rest of the book. It is as though the editor wished to persuade us that the appearance of design in his compilation was quite accidental. But perhaps indeed there was no editor.

C. D. DARLINGTON

"Pop" Propellants

Ignition!—an Informal History of Liquid Rocket Propellants. By John D. Clark. Pp. xiv+214. (Rutgers University: New Brunswick, New Jersey, March 1972.) \$10.

THE title of this book is a little misleading, for the dominant topic is propellant chemistry rather than the "ignition" problem, and in my view the suggestion that the contents are "informal" is an understatement. The style is often personal to the point of

embarrassment, though it certainly conveys the atmosphere behind the security curtain which usually screens this field of science from the general public.

The author, Dr Clark, was a senior propellant chemist with the United States Navy and Army for more than 20 years and is well qualified to explain the development of various rocket propellants from contemplation to completion or more frequently catastrophe! In many instances the workers are in an exasperating situation which, as usual, is funny to the onlooker (remember the case of George's shirt in *Three Men in a Boat?*), and while reading these sections I received many doubtful glances from fellow rail passengers. In order to follow large parts of the text, a knowledge of organic chemistry to at least "A" level is required, and the layman would have to be particularly interested in rockets to plough through all the material. The book should, however, be read by anyone with a degree interested in entering the aerospace, propellant or explosive fields, since it contains a lot of background information which is difficult to obtain elsewhere.

Students of the history of science will find the content strongly biased to the American achievements, and even these are regarded from the service rather than the university or industrial point of view.

The presentation is generally pleasing, with few typographical errors. However, I would have appreciated more photographs and diagrams.

In conclusion, the publication of this book gives an intriguing insight into the propellant community upon whom it undoubtedly exploded.

J. SWITHENBANK

Mathematical Russian

Russian for the Mathematician. By S. H. Gould. Pp. xi+211. (Springer: Berlin and New York, 1972.) 27.60 DM; \$8.80.

THIS book is the outcome of a project, sponsored by the American Mathematical Society, for a "crash course" in Russian, precisely adapted to the needs of mathematicians.

In the resulting course, compiled during a nine-months leave of absence from his duties as Editor of *Translations for the American Mathematical Society*, Dr Gould has approached a difficult task boldly, and with due attention, not only to the linguistic needs of his potential readership, but also to their intellectual aptitude. Basing his method on the assumption that "the vocabulary of the Russian language . . . makes a particular appeal to the

systematizing mind of the mathematician", he introduces vocabulary on the basis of groups of cognate words, explaining their derivations and connexions in an illuminating and memorable manner—if with occasional side-tracking into anecdotal material, or the considerations of historical and comparative phonology, which, although diverting and instructive, seem a little out of place in a brief course of this kind.

The lexical and grammatical material is presented in a clear and orderly manner, and, except for a few *exotica* in the early pronunciation exercises (the probability of *kot*—tomcat, *žuk*—beetle, *žoh*—swindler, *trup*—corpse or *yut*—quarterdeck, occurring in mathematical papers seems extremely small!) the grammar is illustrated by examples such as "the problem reduces itself to such a choice of the function for which the integral has the least value" or "we shall explain some questions of the theory of generalized functions constructed by S. L. Sobolev and L. Schwartz". There are, unfortunately, a few slips, perhaps the most serious being the statement that the genitive of negation is an optional, not an obligatory, construction.

Again catering to the specific needs of his readership, who will, presumably, have their attention drawn to Russian mathematical papers by a notice in *Mathematical Reviews*, Dr Gould recommends the system of transliteration favoured by that journal (although it differs somewhat from both the international and standard American versions). Less happy is his suggestion that the student should "invent" names for those letters of the Cyrillic alphabet which have no English equivalent. It would surely have been far more beneficial to have taught the Russian names for letters—one can visualize a situation of considerable confusion, if, say, in an enquiry by telephone to a library, a follower of Dr Gould's suggested system attempted to spell out a title, saying "zee-check" for "zhe", or "eshch" for "shcha".

In general, however, this book tackles a difficult task straightforwardly and competently. Its selection of texts for continuous reading advances from elementary analytical geometry and calculus, through number theory, rings and fields, to Baire classifications, mathematical logic, Hilbert space and topology. Little provision, however, is made for the applied mathematician, save for one elementary text on the addition of vectors and a short passage on partial differential equations. In view of the vast literature on applied mathematics currently being produced in the Soviet Union, this is, perhaps, the most serious deficiency of the book.

VERA RICH

Dyslexic Disabilities

Dyslexia and the Individual. (A Study of Reading Difficulty in "Word Blind" Children.) By Patrick Meredith. Pp. 190. (Elm Tree: London, November 1972.) £2.35.

It would be easy to find fault with this book. There is no systematic marshalling of evidence, nor do any very firm conclusions emerge at the end. Clearly, however, Professor Meredith did not intend it to be that kind of book. What is being offered is not a systematic treatise on dyslexia but some personal reflexions.

Two main themes emerge. In the first place Professor Meredith delivers a blistering attack on traditional psychometrics (i.e., the alleged "measurement" of IQs, reading ages, and so on). He quotes with approval Eddington's dictum that "we can make nothing of measures without any note of the objects and circumstances to which they refer", and he points out that, when a child is being tested, the "circumstances to which (he) is responding . . . usually include a great deal more than the particular test-items on which the psychologist is concentrating". His second theme is that we should try to become more sensitive to what happens when people are exposed to different spatial and temporal sequences. "Laterality", he tells us, "is full of surprises".

The attack on psychometrics seems to me undoubtedly correct. I also agree that dyslexic children often fail "to co-ordinate, in time, their perceptions of space". There is still much in the book, however, which I find obscure, and there is even a certain amount which I suspect to be nonsense; for example, "The space-time of the child is a field of events, curled up and carved up in the convolutions of his cortex". On page 77 he promises a further book devoted to "orthochorics", which he refers to as a kind of "behavioural geometry". From the present book the case for such a science is by no means made out, but here one must suspend judgment until more details are published.

I have two further comments. First, it seems to me misleading that the book has been given the sub-title, "A Study of Reading Difficulty" (my italics). Professor Meredith would, I am sure, be the first to agree that for many children with the dyslexic cluster of disabilities reading is the least of their problems; and I think it should be stressed that, in spite of the incorrect associations with the Latin word *lego*, dyslexia is not just a difficulty with reading. Secondly, I am surprised that Professor Meredith shows so little enthusiasm for the work of Skinner. The "cumulative record", used by

Skinner and his followers to provide a spatial representation of an organism's behaviour over time, is just the kind of notation which he might have been expected to favour, while Skinner's concept of the "discriminative stimulus" is one which forces experimenters to take serious note of the particular "circumstances" in which an organism's responses occur. Professor Meredith's views seem to me to have more in common with those of Skinner than perhaps he realizes.

T. R. MILES

Radiation Protection

An Introduction to Radiation Protection. By Alan Martin and Samuel A. Harbison. Pp. xi+216+4 plates. (Chapman and Hall: London, October 1972.) £1.95.

THIS book is intended to introduce those who have a science education to "O" level or equivalent standard to the principles and practice of radiological protection. Those persons most likely to find the book of value are trainee technicians and health physics monitors who will become responsible for the day-to-day control of radiation hazards in nuclear power stations, research establishments, hospitals and industrial premises where employees are exposed, or potentially exposed, to ionizing radiation.

The book contains seventeen chapters and at the end of each there is a summary and a number of revision questions which could be useful for both student and teacher at the level of City and Guilds examinations. The first few chapters deal with elementary theory of the structure of matter, radioactivity and radiation units. The middle chapters are on protection standards and methods of monitoring. The later chapters introduce practical aspects such as waste disposal, legislation, transport regulations and radiation emergencies, and these will be of particular interest as this material is not readily available in such an easily readable form.

Two chapters are of lower quality than the rest, one on "Biological Effects of Radiation" and the other on "The Internal Radiation Hazard". The authors could improve the former by the use of more up-to-date material from the excellent summary of the subject published as *Radiobiological Factors in Manned Space Flight*.¹ The latter is too short for such a complex topic. Its readability could be improved by a more liberal use of diagrams to illustrate metabolic pathways for ingestion and inhalation. This chapter also contains some errors of fact. In one

place, for example, bone is stated to be the critical organ for inhaled plutonium but this is not the case for insoluble compounds. At another point the wildly erroneous statement is made that "the rate of excretion of any substance from the body is approximately exponential". Here the desirable mathematical model seems to have been substituted for reality.

Apart from these small criticisms the book is entirely satisfactory and can be recommended to those teaching or learning elementary radiological protection theory and practice.

G. W. DOLPHIN

¹ Langham W. H. (ed.), *Radiobiological Factors in Manned Space Flight* (National Academy of Sciences, Washington DC, 1967).

Temporal Processes

What is Time? By G. J. Whitrow. Pp. 191. (Thames and Hudson: London, September 1972.) £2.

VIEWED simply as a popular exposition of a complex and difficult scientific theme, Professor Whitrow's latest book—an expanded version of four BBC Third Programme talks on the nature of time—is a remarkable *tour de force*. Its topics range from the Mayan conception of time to the ideas of Einstein and Minkowski, from water clocks to atomic clocks, from the fossil record to human memory, from biological evolution to cosmic evolution, from ancient religious beliefs to modern philosophical doctrines. Professor Whitrow has managed to combine these and other facets of his vast subject into an integrated whole in which each facet illuminates and is illuminated by the others.

But this book is more than an extraordinarily successful popularization. It is also a perceptive historical account of the biological, social, religious and philosophical influences that have shaped Western ideas about time and temporal processes. Throughout most of human history time has been perceived as being essentially cyclic. In man, as in other organisms, diurnal, monthly and annual rhythms have become internalized through evolution. Moreover, except in highly industrialized societies, everyday life has always been dominated by the cycle of day and night, the ebb and flow of the tides, the progression of the seasons, the life cycles of plants and animals. The cyclic view of time was further reinforced by observations of planetary motions, which were thought—and are still thought by many—to govern the subtler rhythms of men and nations. With the rise of Newtonian mechanics and of the technology that sprang from

it, Newton's view of a universal time, flowing "equally without relation to anything external", gradually displaced the ancient notion of cyclic time. Yet the Newtonian vision of the world differs almost as much from the modern one as from the one it displaced, for Newton believed that the world and its creatures had been shaped and set in motion by the hand of God only a few thousand years earlier. The modern view (still fragmentary and incomplete)—that the physical universe and the biosphere are the results of evolutionary processes—began to take shape only toward the end of the 18th century, and only during the last few decades have we been able to make reliable estimates of the time scales involved. Despite its brevity, Professor Whitrow's account of these developments, illustrated by aptly chosen quotations from literary, philosophical and scientific sources, is clear and well balanced.

On a third level, the book addresses itself to abstract philosophical doctrines. Professor Whitrow's skill in extracting the essence of a philosophical argument and presenting it in clear, simple language is reminiscent of Russell's. His critical technique relies heavily, and to excellent effect, on confronting doctrines in "pure" philosophy with relevant empirical evidence and theories from psychology, biology and the physical sciences. I especially enjoyed (though I was not always in full agreement with) the critique of Kant's views, the comparisons among absolute time, relational time and relativistic time, and the discussion of the transitional nature of time and the failure of current theories to come to grips with it. Professor Whitrow summarizes his own philosophical position in the book's final paragraph: "Although our perception of time has many subjective and even sociological features, it is based on an objective factor that provides an external control for the timing of our physiological processes. This objective factor is what we call physical time. It is an ultimate feature of the universe and its relationship with observers, particularly fundamental observers, which cannot be reduced to anything else. But this does not mean that it exists in its own right: it is an aspect of phenomena. The essence of time is its transitional nature. That this has given rise to so much argument down the centuries is not surprising, for, in the words of Whitehead, 'it is impossible to meditate on time and the mystery of the creative passage of nature without an overwhelming emotion at the limitations of human intelligence'."

Because of its scope, depth, readability and scholarly quality, this book, though intended for the general reader, is also admirably suited to serve as a text for an undergraduate seminar. Its

value in this capacity would be enhanced by the addition of some bibliographical notes and a list of references, which, it is to be hoped, will be added in subsequent editions.

DAVID LAYZER

Spectroscopy

Spectroscopy. Edited by D. A. Ramsay. Consultant editor, A. D. Buckingham. (MTP International Review of Science. Physical Chemistry, Series One, Volume 3.) Pp. 338. (Butterworth: London; University Park: Baltimore, Maryland, 1972.) £10; \$24.50.

THIS volume is one of the first of a new series of comprehensive scientific review volumes produced by the Medical and Technical Publishing Company. Chemistry as a whole is covered in thirty-three volumes; this is volume 3 of the thirteen volumes devoted to physical chemistry. It consists of eight chapters of about forty pages each, these being independent reviews by distinguished contributors of various special fields of current interest.

The book is an undoubted success. All of the authors speak with authority on their subjects, and the subjects have been well chosen, although I am sorry that there is no chapter reviewing some of the more exciting experiments being done currently with lasers, double resonance spectroscopy, and suchlike. The reviews are indeed comprehensive, and in most cases report developments of the past ten years (rather than the last two years) in the field reviewed. Most of the readers will already be spectroscopists of one form or another. The volume has more in common with the series *Advances in Spectroscopy* edited by H. W. Thompson and published by Interscience, which ran to only two volumes (1959 and 1961), than it has with *Annual Reviews of Physical Chemistry* or *Annual Reports of the Chemical Society*. Its closest competitor today might be the appropriate volume of the Chemical Society *Specialist Periodical Reports*. The book is well produced, although it is quite expensive.

It is at present planned to republish the entire series every two years. If this plan is maintained, I think the style of the articles is likely to change from those in the present volume to something more akin to a catalogue of recent progress; I would regret this change, since such articles are less valuable and already exist in various rival publications. Either the subjects reviewed in the next volume should show little overlap with those reviewed here, or an interval of five or six years would be more appropriate if the style of the articles is to be maintained.

IAN M. MILLS

Non-molecular Solids

The Structures of Non-molecular Solids: a Coordinated Polyhedron Approach. By Graham M. Clark. Pp. xii+365. (Applied Science: London, 1972.) £9.

THIS book contains some short introductory chapters on sphere packing, crystal geometry, coordination polyhedra, and defect solids, and a short final chapter on lattice energies. Discussion of the relation of physical properties to structure is qualitative and on an elementary level. The principal part of the book is an account of the crystal structures of inorganic compounds (predominantly oxides and halides) arranged according to the way in which the coordination polyhedra (including planar triangular and square groups) share vertices, edges, and/or faces. Most of the structures are also described in terms of the closest packing of the larger ions. The book is necessarily concerned largely with structures based on tetrahedral and octahedral coordination and, in spite of the title, some molecular crystals are included. On the other hand, structures of very low coordination are not described in terms of the simple 2- and 3-dimensional nets which provide such elegant descriptions of a number of crystal structures. The omission of the simplest 3D 4-connected net leads to the description of the diamond structure as "that of sphalerite, except that carbon atoms occupy both the Zn and S sites". There are descriptions of a few structures of coordination number greater than 6, but the book does not cover the coordination numbers greater than 12 characteristic of many intermetallic compounds.

In general the descriptions are accurate and they are illustrated by many diagrams, some of which present novel views of the structures; a few should be redrawn. The grammarian might object to the statement that "one of the most polymorphic substances known is silica"; objection should be taken to the descriptions of K_2SbCl_5 and K_2PdCl_4 as defect structures because this term is used in a different sense in chapter 4, and to the description of Fig. 1.15 (which should be rotated through 90°) as "The true (rhombohedral) unit cell of the ccp (fcc) arrangement". The opening sentence of the book: "High density and symmetrical external shape are two characteristic physical properties which distinguish crystalline solids from other states of matter" illustrates the dangers of generalizations.

Since this book is largely developed from other texts and secondary sources (though some references are given to the original literature) we must consider the merits of its characteristic feature,

namely, the systematic description of structures in terms of the way in which the coordination polyhedra are linked together. First, there are three types of model (or illustration) that can be made of many simple inorganic structures, the ball-and-spoke, close-packed sphere, and polyhedral (coordination group) model. Exclusion of the first of these leads to clumsy descriptions of certain simple structures, for example, that of diamond noted above, the NaCl structure as one in which octahedral coordination groups share all twelve edges, and the antiperovskite structure as one in which tetrahedral groups share all six edges. Certainly structures such as those of ZnS, PtS, and NbO are more simply described in terms of 3D nets than as assemblies of coordination polyhedra. Second, the description of structures in terms of the coordination polyhedra around the metal atoms (ions) tends to minimize the importance of the environment of the anion, a shortcoming which is also a feature of many descriptions of structures in the original literature. Third, the strictly systematic geometrical approach to structures has additional value if we wish to compare the geometrically possible structures with those actually found, and to discuss why certain structures are not adopted. Conversely, the (geometrical) impossibility of building structures for compounds A_mX_n from certain coordination polyhedra would seem to be the reason for some of the "anomalies" which puzzle the author in applying the radius-ratio concept to simple ionic crystals, to the structures of which it has, in fact, only a very limited relevance. Finally, most readers, whether students or teachers, find difficulty in visualizing many structures from plane illustrations, however well they may be drawn, and I am rather doubtful about the value of the polyhedral representations of many of the more complex structures.

Although I look upon the material of this book as an integral part of structural inorganic chemistry, this viewpoint is not yet universally adopted, and it is unlikely that the book will be acceptable at the present time as an undergraduate text. It is my opinion that a real understanding of the geometry of crystal structures can be gained only from the study of models, preferably built by the student. This book could provide useful supplementary reading in places where the teaching of inorganic chemistry has advanced to the stage of including model building as part of the practical curriculum. Unfortunately, even after sixty years of X-ray diffraction many teachers of chemistry do not appreciate, or are unwilling to acknowledge, that the structures and

properties of solids form a large and important part of inorganic chemistry, though the inclusion in the syllabus of structural organic chemistry of comparable complexity is not questioned. An early introduction to repeating patterns and some solid geometry would help to produce a generation of teachers willing to venture beyond the familiar finite groups of atoms.

A. F. WELLS

Lunar Data

The Moon. Edited by S. K. Runcorn and H. C. Urey. (International Astronomical Union Symposium held at the University of Newcastle upon Tyne, March 1971.) Pp. xvi+471. (D. Reidel: Dordrecht, Holland, 1972.) Dfl. 110.

THIS is the report of the second IAU symposium to be devoted to the Moon. While it was not restricted to results gained by space technology, comparison with the first symposium held ten years earlier does show how the subject has developed out of all recognition in consequence of the various Luna and Apollo missions. The growth is alarming, but the editors do call attention to the agreeable feature that it offers to graduate students in the physical sciences a great opportunity to share in interpreting this vast corpus of information. Students who seize this opportunity can have the assurance that they are working on problems of rewarding significance.

The book comprises well prepared and presented texts of most of the papers read at the meeting. There are forty of these, classified into nine sections dealing, in brief terms, with lunar mechanics, lunar surface, Apollo missions, petrology, tectonics, physics of lunar samples, interior, orbit, origin and evolution. A majority of the papers report very recent observational and experimental work and its fairly immediate interpretation. Only a few deal with more general theoretical problems, chief amongst these being the papers by Urey and Levin who discuss the origin and evolution of the Moon, or at any rate survey the significance of new discoveries for these topics. Urey remarks, "No one has changed his mind in regard to the origin of the Moon, and we can only conclude that all the evidence from the space programme is indecisive in regard to this question". This sums up the impression one forms for oneself on reading this admirable compendium of so much of that evidence. However, the fact that such a great deal has been assembled in one volume of manageable size should itself help those who are concerned with the larger problems to discern the more general significance of the evidence, and so maybe to make better use of it.

In this connexion one might suggest that any future compilation of this sort might well include a glossary designed to help the more general reader to understand the language of the specialists in each section.

The attractiveness of the book is much enhanced by the frontispiece—a charming snapshot of the founding fathers of the modern study of the Moon, A. A. Mikhailov and H. C. Urey. W. H. McCrea

Regenerative Capacity

Organ Regeneration in Animals: Recovery of Organ Regeneration Ability in Animals. By L. V. Polezhaev. Pp. ix+190. (Charles C. Thomas: Springfield, Illinois, 1972.)

POLEZHAEV is best known for his early work in which he persuaded frog limbs to regenerate after amputation. Normally tadpoles can, and adults cannot, perform this feat. To restore the capacity lost in ontogeny he, Rose, and Singer independently used different methods, but each with some success. It is natural to turn from such work to other systems in which regenerative capacity is absent or limited, to apply the lessons learned. Two such systems loom large in this book—the irradiated urodele limb, and mammalian tissues. It is claimed that the former can recover regenerative capacity after the administration of several substances—rat muscle homogenates, rat liver RNA, or milk protein.

The author reviews recent work, much of it from the USSR, on mammalian tissues, for the benefit of clinicians who are, of course, directly concerned. Unfortunately, his treatment is neither entirely summary nor sufficiently detailed to allow the reader to judge the experimental evidence to which he refers. The section on the central nervous system with which the work ends is particularly difficult to assess, since the claims to have induced DNA synthesis, nuclear division, and cell division (as well as endopolyploidy) in mammalian cerebral neurones by treatment with tissue extracts are uncritically presented.

Above all, however, the editing of the English version of this book is so awful that its message can only be approached through a haze of words misspelled, misused, or simply invented. The syntax also is odd. In the interests of rapid, or of cheap, publication of translated texts one should, in my opinion, be tolerant in such matters, but this book goes too far. The reader's irritation transfers to the substance of Polezhaev's arguments, some of which certainly need what help they can get from a faultless exposition.

D. R. NEWTH

Magnetic Resonance

Magnetic Resonance. Edited by C. A. McDowell (MTP International Review of Science. Physical Chemistry, Series One. Volume 4.) Consultant editor, A. D. Buckingham. Pp. 365. (Butterworth: London; University Park: Baltimore, Maryland, 1972.) £10; \$24.50.

HERMES TRISMEGISTUS, who knew everything and, it is said, wrote it all down in 36,525 volumes, is to be emulated a few thousand years later by the MTP International Review of Science, of which the present book is volume 4 of the first series for the treatment of physical chemistry. Meanwhile, the theory and practice of magnetic resonance have developed notably, and the editor (who now, alas, is needed) has attempted to provide a glimpse of the advances in the subject between 1967 and mid-1971. The ten chapters cover nuclear spin relaxation in gases (Bloom), n.m.r. studies of molecular motion in solids (Allen), Mössbauer spectra (Sams), n.q.r. (Chihara and Nakamura), ^{13}C relaxation (Lyerla and Grant), liquid crystal spectra (Bulthuis, Hilbers, and MacLean), electron resonance in gases (Brown), optical detection (Kwiram), irradiated organic crystals (Iwasaki), and the biological applications of e.s.r. (Bolton and Warden).

Many of the topics treated correctly reflect the modern tendency in magnetic resonance towards either the study of physical phenomena or biological systems; the volume teeters on the brink of ecodoom in only one place (page 351). Nevertheless, there are topics omitted whose inclusion would have made a truer representation of the present activities: Fourier transform techniques in n.m.r., n.m.r. of biological systems, and chemically induced spin polarizations are not included. The authors have written in a variable fashion; most chapters have introductory material, but vary in the rapidity with which they become synoptic reviews. It is important for the editors to establish whether the contents are to be comprehensive and detailed, or essays on topical subjects; since the *Specialist Periodical Reports* of the Chemical Society attempt the former, and more cheaply, I would prefer the second alternative. There is no reason why both cannot be combined: a review of the principles and status of the subject could be followed, but clearly distinguished from, a thorough and comprehensive specialized review of its progress. In any case, the non-specialist reader would be helped if in future volumes each chapter contained a synopsis of the most notable advances that had been made, and a brief résumé of the present centres of interest.

The volume has neither author nor subject index, although we are promised one for the whole set. This omission is inexcusable and suggests quite clearly that the books are aimed at libraries whose hitherto generous bosoms can embrace complete sets. Private bosoms, who might muster £10, will, and should, feel cheated.

P. W. ATKINS

Logic of Mathematics

What is Mathematical Logic? By J. N. Crossley, C. J. Ash, C. J. Brickhill, J. C. Stillwell and N. H. Williams. Pp. ix+82. (Oxford University: London and New York, November 1972). £1.40 cloth; 70p paper.

THIS little book grew out of a short course of lectures to non-specialists at Monash and Melbourne, and it retains much of the freshness of that venture. The tone is set by the authors' opening declaration: "Mathematical logic is a living and lively subject. We hope that this will be conveyed by the somewhat unconventional style in which this book is written." On the whole, books on mathematical logic tend to be either systematic textbooks, which their readers are meant to work through slowly and methodically, or else more or less perfunctory popularizations of the subject; but the present authors have attempted something rather different, which should be of substantial benefit both to logical studies and to education as a whole. In a brisk review of mathematical logic, covering all its main branches, they give a succinct account of the central problems and results, in which, despite the informality of their style, they treat seriously the ideas and methods that really matter. Non-logicians to whom abstract thought is not entirely foreign should thus be able to gain some knowledge of what kind of subject mathematical logic is, and of where it is located on the academic map; while readers who subsequently embark on serious study of this subject will do so with an initial orientation to assist them in finding their bearings. For those who are inspired to read on further for themselves, a helpful reading list is provided.

The book comprises an introductory historical survey (from about 1850 to 1963), followed by separate accounts of the completeness of the predicate calculus, model theory, Turing machines and recursive functions, Gödel's incompleteness theorems, and set theory. As might be expected, the chapters are not all equally successful; but the best of them—particularly the ones on Turing machines and Gödel's theorems—are very readable and informative.

G. T. KNEEBONE

CORRESPONDENCE

Bad BBC Science

SIR,—Since I advised on the Horizon programme "Science is Dead—Long Live Science" variously described in your leader (*Nature*, **241**, 490; 1973) as "disastrous", "witless", and "dangerously infected with heresy", I would be grateful for the opportunity to make a few comments.

First, I must remark on the distressingly high proportion of gross errors of perception on which your writer based his judgments. The Vietnam films were very far indeed from "the most gruesome of the gruesome"; the "defoliant" scientist was interviewed but not the "napalm" scientist; nowhere was there a claim that science made the Vietnam war possible; the scientist-turned-printer was not "offended" by the FBI, but described quite clearly how he was hounded out of his job; the interview in West Virginia had as its whole point the complete corruption of the local government by the strip-miners and the effective disenfranchisement of the public; Jonathan Beckwith neither gave alarming tales of genetic engineering nor said when civilization would end; the New Alchemist made no claim to secret knowledge; and it was a quite different group who have gone over to Sun worship.

Perhaps there were some deeper failings in presentation that enabled your reviewer to become so confused. If so, I and the producers would be very grateful for help. It seemed abundantly clear to us that each of the very different sorts of scientists presented in the programme were a small minority—the general indifference at the AAAS meeting was, we thought, a sufficient reminder of this. We thought also that the difficulties and contradictions in each reaction, systematically and sympathetically described by Beckwith, were made clear in nearly every case.

If I were to help in modifying the programme for a re-issue, I would start by putting more emphasis on one particular still shot. This is of a proud advertisement by North American Rockwell. It describes their development work on the B-1 bomber, the planned successor to the B-52 of saturation-bombing fame. Their triumphant slogan is "North American Rockwell—where Science Gets Down to Business". This phrase, so cleverly combining a straight description with an American idiom, has rich overtones in the political and moral dimensions. I strongly suggest that your reviewer reflect on that slogan; from

that could come an appreciation of what the programme was all about.

Finally, I hope that your reviewer will, on reconsideration, regret that personal sneer at the physicist persecuted by the FBI, and also the most unfortunate arguments on the responsibilities of civil servants by which the reviewer justified himself. This lent neither strength nor dignity to his criticisms of the programme, and a retraction and apology would seem appropriate.

Yours faithfully,

J. R. RAVETZ

Department of Philosophy,
University of Leeds

SIR,—It is unfortunate that your attack (*Nature*, **241**, 490; 1973) on the BBC for irrationality in science programmes was itself so irrational. It used a device much favoured in debating societies, but surely to be abjured by a scientist with any pretensions to objectivity, namely falsely accusing the opposition of having made a certain statement and then proceeding to demolish it.

In stating that the Horizon programme "Science is Dead—Long Live Science" implied an absurdity, namely that the profession of science, not the political climate, made the Vietnam war possible, you underestimate the average viewer's intelligence and good sense and negate your own. "Implication" is in the brain of the beholder, and I for one caught no shadow of such an implication. The programme was making a quite different point, clear enough to an unbiased viewer, that the scientific weapons being used were based on investigations made for peaceful ends, not specifically developed by government scientists aware of their mission. Almost all your allegations are similarly off beam. To take just one more example, it is simply not true that the programme used Beckwith "as an excuse for alarming its audience with tales of genetic engineering". The programme was, in fact, making exactly the opposite point, namely that when Beckwith tried to interest the community in a (note) rational consideration of the possible consequences of chemical synthesis of a gene, he was so distressed by the alarmist furore that his action set off in the popular press that he desisted.

What impels me to write, rather than ignore such ill-founded criticism, is that your editorial castigated a courageous and original attempt to explore a real and important facet of the scientific world, namely the constant and insidious

worry about the hideous uses to which the most innocent scientific discovery may eventually be put. What gives Horizon programmes their continuing vitality is precisely these qualities of courage and originality. It is easy to make programmes which are aimed at blinding the public with science and how marvellous it all is; it is easy too (as your editorials frequently demonstrate) to rail at the Establishment, and even easier to ridicule the thoughtful and rebellious young. It is not so easy to describe, quietly and objectively, an uncomfortable phenomenon in our midst—scientific heretics—and to discuss without hysteria a profound problem that troubles us all; whether to continue to wrest secrets from nature that may be perverted to evil ends. In my opinion, this Horizon programme did just that, and did it extraordinarily well.

Yours faithfully,

F. PETER WOODFORD

Creation vs Evolution

SIR,—The decision of the State Board of Education in California to preach the doctrine of creation rather than to teach the theory of evolution in the state high schools has not only created a stir among the intellectuals of the world, but thrown a challenge to the entire scientific world to dispel myths about certain phenomena of the universe which are considered as status symbols of transcendental edicts of present day society. The letters of G. Vanderkooi, E. C. Lucas, and A. R. Smith (*Nature*, **240**, 365–366; 1972) and A. J. J. Hayward (*ibid.*, 492) support the existence of supernatural power which controls the universe. It is unfortunate that a plant taxonomist, A. R. Smith, could not visualize the process of evolution, which he ridiculed as a "tissue of lies". I, as a botanist, do not agree with him.

The concept of evolution is based on scientific facts, and reveals the truth of nature, although problems such as the origin of life and man's origin are still open for further studies. It is necessary to realize that evolution does not always mean the tracing out of the ancestry of organisms, but as a natural process that has been operating in the universe. Within the living organisms it is an essential natural process taking place as a result of interaction between the genomes and the ever changing environment. Genetic recombination, mutation and selection are the main wheels on which the vehicle of evolution is

proceeding to its never ending destination. Vanderkooi, who argued that there is no evidence to account for the process of macroevolution, is probably unaware of the fact that adaptive radiation is responsible for the evolution of higher taxa. True, there are certain forces in nature which cannot be readily explained in terms of science. This inability of science has often been taken up as a proof of the existence of a creator. It is increasingly evident that selection pressures direct the process of evolution. Although it is very difficult to interpret what is exactly a selection pressure, it is not ruled out that a scientist may come out with a model which unearths the hidden secret of selection pressures.

Vanderkooi pointed out that bacterial cells are as complex as any other living cells of the organism, and as such they cannot be considered as the primitive living organisms from which other organisms have been evolved. It is possible that all living organisms might have evolved from some primitive form (monophyletically), and further diversification within the evolved groups might have resulted in the evolution of different groups polyphyletically. The closer resemblance of the amino-acid sequence in cytochrome-C of wheat to that of animals rather than to cytochrome-C of fungi favours the polyphyletic origin.

On the other hand, the concept of

creation is nominalistic, and has no place in scientific thinking. The doctrine of creation is a man-made catastrophe, and if it is continued to be advocated by man as a basic conceptual controversy, it is just like putting the clock back and will hamper the advancement of humanity.

To denounce the teaching of the theory of evolution, which has been nurtured by the cumulative efforts of hundreds of scientists for about 100 years, is to deny the legitimate rights of society to learn and understand the reality of the nature and the role of man as part of the natural and social world. If it is done it will become one of the greatest tragedies and educational failures of our time.

Yours faithfully,

C. R. BABU

Department of Botany,
University of Delhi,
Delhi-7

Use of IS, TWA and ILWS

SIR,—The present author (PA) is writing to request the increased use in *Nature* of initial-letter-word-substitution (ILWS). Unpublished studies of the PA show that the superficial-scientific-appearance-rating (SSAR) of a paper is most effectively increased by impersonal style (IS), technical-word-

amplification (TWA) and ILWS. There is, moreover, a highly significant positive correlation between the SSAR of a paper and its peer-esteem-rating (PER). The PA hopes that *Nature* will co-operate in raising the PER of its contributors by encouragement of ILWS, as well as acceptance of IS and TWA.

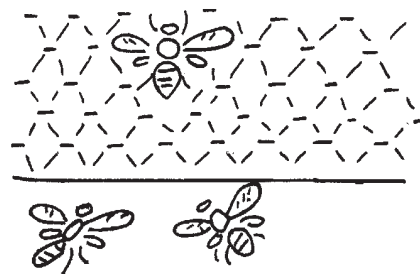
Yours faithfully,

J. M. BARRY

Department of Agricultural Science,
University of Oxford,
Parks Road, Oxford OX1 3PF

Between Bees

SIR,—



He's saying, "Go to Hive 2 tomorrow. They're killing bees at Hives 1 and 3".¹

Yours faithfully,

F. J. MALONEY

Livermore,
California

¹ *Nature*, 241, 171 (1973).

Obituary

Dr Stephen R. Pelc

STEPHEN R. PELC, who died suddenly on February 6, at the age of 65, was internationally famous for his pioneer work on the techniques of autoradiography and for his use of these techniques in cellular biological research. He graduated from the University of Vienna and published his first work from the Institut für Radiumforschung in 1931. In 1938 he came to Britain and, after serving in the forces during the war, he restarted his scientific work at the Hammersmith Hospital, London, first as a hospital physicist and then as a member of the MRC Radiotherapeutic Research Unit.

There he began his studies on the action of ionizing radiations on photographic film which led to his development of stripping-film autoradiography for obtaining high resolution autoradiographs of sections of biological material, work done with the enthusiastic collaboration of Dr (now Professor) I. Doniach and under the scientific directorship of the late Dr L. H. Gray.

Dr Pelc had the idea of using pliable photographic film stripped from Ilford stripping plates which were, at that time, being used in industry for other purposes. Kodak then produced the AR10 and AR50 stripping film to his requirements, giving far greater resolution, so that these have now become the standard materials for this form of autoradiography. Initially, when ¹³¹I was the most readily available isotope, this technique was used by Professor Doniach and himself for studies on the thyroid gland under various physiological conditions. As other radioactive isotopes became available, however, the applications of autoradiography expanded rapidly so that it is now used throughout the world on many varied biological and medical research problems.

He is most famous for work begun in the early nineteen-fifties with Dr Alma Howard. By incorporating ³²P into dividing cells and removing all but the DNA by acid hydrolysis he was able to time the incorporation of ³²P into nuclear DNA. He showed that DNA synthesis did not occur after prophase,

as had been believed previously from staining evidence, nor did it occur continuously throughout interphase. He and Howard showed that, for each type of nucleus, there was a particular period of interphase, which he called the "S" (synthesis) stage, during which the DNA content doubled; this DNA was stable and became divided equally into the two daughter nuclei. Before and after the "S" there was a gap in his knowledge of what metabolic processes occurred in the nuclei and, understandably, he named these "G₁" and "G₂". The period of interphase therefore could be extended if G₁ or G₂ became prolonged. These results were confirmed and extended considerably with the introduction of other isotopically labelled DNA and RNA precursors, particularly those labelled with ³H.

Thus he laid the basis for the study of cell kinetics based on the timed uptake of isotopically-labelled precursors and their distribution into the cells derived from the initial, labelled mitotic cell. Of particular importance were his studies on the small intestine, in which

there is a constant replacement of cells, and on the skin where the processes of keratinization affect the DNA of the daughter cells.

Dr Pelc collaborated with many major research workers in autoradiographic metabolic studies on their own research problems. Thus his own interests extended from DNA over much of metabolic cellular biochemistry. By 1957, however, when he moved to the MRC Biophysics Research Unit at King's College, his main concern had again become DNA. He then showed, in a series of elegant studies, that the DNA of a wide variety of cells was not as completely stable as had been thought originally during the first flush of the DNA-gene concept. His work showed that sometimes as much as 50 per cent of the nuclear DNA was being turned over, the proportion of this "metabolic DNA" in any given nucleus apparently varying with the general metabolic activity of the cell.

For the past two years he was a member of the MRC external staff, working in the Division of Cellular Biology of the Kennedy Institute of Rheumatology in London. Here he extended his work on metabolic DNA, and showed that the metabolic DNA of splenic lymphocytes was considerably stimulated as a result of the immune response. He had also shown that metabolic DNA was of considerable significance in the process of ageing, and was currently very interested in the work of his collaborators, Dr M. Stroun and Dr P. Anker (Geneva) which showed that DNA can be passed from cell to cell. Thus it seemed likely to him that the metabolic DNA, which he saw as the expendable copies of the genic DNA, could act as a messenger between cells and so carry active gene messages to cells in which these genes were otherwise repressed.

Stephen Pelc was an outstanding scientist. A physicist by training, he was a cell biologist first and foremost, who used physics and mathematics adroitly in his biological research. Everyone who knew him was impressed by his gentle, genial kindness, his readiness to help others and his good humoured philosophical approach to science and to life generally. He was an original thinker who designed his experiments decisively. Thus he was a pioneer, and pioneers rarely get the immediate recognition of their innovations. Sufficient time had elapsed from his development of autoradiography and his work on the timing of the nuclear synthesis of DNA for him to have been recognized as the authority on the techniques and on cell kinetics. The full significance of his discovery of the metabolic turnover of a fraction of nuclear DNA may yet take several years.

Second to science, his major interest

was music, in which he found relaxation and mental refreshment. He was an accomplished violinist and leader of a string quartet which met regularly. He will be greatly missed by his fellow scientists who relied so heavily on him for his general scientific wisdom as well as for his specialized knowledge.

Announcements

Miscellaneous

The following have been elected **Fellows of the Royal Society**: **Professor Percival Allen** (University of Reading); **Dr Brigitte Alice Askonas**, (Immunology Division, National Institute for Medical Research, London); **Mr Francis Thomas Bacon** (Fuel Cells Limited of Cambridge); **Dr Alan Baker** (University of Cambridge); **Professor Neil Bartlett** (University of California at Berkeley); **Professor William John Granville Beynon** (University College of Wales, Aberystwyth); **Mr John Gatenby Bolton** (Australian National Radio Astronomy Observatory at Parkes, New South Wales); **Professor David Roxbee Cox** (Imperial College of Science and Technology, University of London); **Professor Leslie Crombie** (University of Nottingham); **Professor Harry Elliot** (Imperial College of Science and Technology, University of London); **Professor Douglas Scott Falconer** (University of Edinburgh); **Professor Geoffrey Alan Gilbert** (University of Birmingham); **Professor Harish-Chandra** (Institute for Advanced Study, Princeton, New Jersey); **Professor Richard John Harrison** (University of Cambridge); **Professor Harold Horace Hopkins** (University of Reading); **Dr Anthony Kelly** (National Physical Laboratory, Teddington, Middlesex); **Dr Egon Hynek Kodicek** (MRC Dunn Nutritional Laboratory, Cambridge); **Professor Jack Lewis** (University of Cambridge); **Dr Mary Frances Lyon** (MRC Radiobiology Unit at Harwell); **Dr Peter Bryan Conrad Matthews** (University of Oxford); **Professor George Francis Mitchell** (Trinity College, University of Dublin); **Dr Helio Gelli Pereira** (Division of Virology at the National Institute for Medical Research, London); **Professor Paul Emanuel Polani** (Paediatric Research, University of London); **Professor John Graham Ramsay** (Imperial College of Science and Technology, University of London); **Mr Lionel Edward Aston Rowson** (ARC Unit of Reproductive Physiology and Biochemistry, Cambridge); **Dr Monkombu Sambasivan Swaminathan** (Indian Council of Agricultural Research, New Delhi); **Dr Jamshed Rustom Tata** (Developmental Biochemistry, National Institute for Medical Research London); **Dr David Warren Turner** (University of Oxford); **Professor William Frank Vinen** (Univer-

sity of Birmingham); **Professor Paul Egerten Weatherley** (University of Aberdeen); **Professor Ronald Whittam** (University of Leicester); **Professor Alec David Young** (Queen Mary College, University of London).

Reports and Publications

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 Griffin and George, Ltd. Apparatus for New or Expanding Laboratories. Pp. 53. Apparatus and Chemicals for the Nuffield Combined Science Course. Pp. 13. Apparatus and Materials for the Science 5/13 Project. Pp. 13. Apparatus and Chemicals for the Scottish Integrated Science Course. Pp. 25. Griffin Brochure, New Year Edition. Pp. 23. (Wembley: Griffin and George Limited, 1972.) [121]
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